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GENERAL SECTION

SOME CHARACTERISTICS OF RECIPROCAL WRIST ACTION

BY E. A. BOTT.
(*From the Psychological Laboratory, University of Toronto.*)

1. *The Psychological Approach* (pp. 1–2).
2. *The Factors under Control* (pp. 2–4).
3. *The Variables Investigated* (pp. 4–6).
4. *Quantitative Results*[1] (pp. 6-16).
5. *Other Results* (pp. 16–24).

1. THE PSYCHOLOGICAL APPROACH.

WORKING with human subjects prevents that precision of experimental control which is enjoyed, for example, by a physiological method which can employ incision and local stimulation. A compensating advantage is that one is compelled to make a direct approach to the question of how certain muscles or muscle groups operate synergically and antagonistically in moving a limb under volitional stimulation. From a psychological standpoint the problem is to find what sort of movements a subject can accomplish with certain muscles as compared with others under controlled conditions.

Unfortunately little advantage can be derived from the large amount of previous physiological investigation[2] upon the maximum work output when a large part of the muscular system operates as in riding a stationary bicycle. Here neither the work output as ergographically measured, nor the accompanying physiological changes, furnish information upon the differential possibilities in co-ordination of the musculature involved. From this 'total work' method one cannot deduce in detail what would result from other muscle combinations or what combinations would be the most effective for particular purposes, *e.g.* for speed or accuracy or force, in a particular type of movement. For such information direct inductive inquiry is desirable, notwithstanding difficulties in the way of precision owing to the many variables involved. Physiological investigation of isolated muscle, however, may furnish valuable deductive evidence upon human muscle action, as Hill[1] has shown, and computations by this method can to a certain extent be checked by experimental tests with human subjects.

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[1] Some of the points in this connexion are still under investigation. For the results presented below the writer wishes to acknowledge helpful suggestions received from Professor Beatty on the mathematical side and also the painstaking services of all those who assisted in the experiment, particularly the assistance of Miss E. Burton and Miss D. D. Hearn in measuring and computing the extensive data.

[2] For example, Benedict and Cathcart, "Muscular Work," *Publication* 187, *Carnegie Institute of Washington*, 1913; Bainbridge, *Physiology of Muscular Exercise*, 1919; Amar, *Le Moteur Humain*, 1914; and others.

The present study is largely a reversal of this mode of approach, its aim being to ascertain the facts about muscle action by quantitative examination of the resultant movement and to follow up the points which the data suggest as significant. Experience in functional re-education of incapacitated muscles had given the writer the impression that reciprocal action is a dominant factor in the majority of purposive co-ordinations. For this reason, rather than because it is doubtful whether the reciprocal relationship can be eliminated in the case of the normal subject, this point is given prominence. Wrist action has been selected for three reasons: (1) the forearm can readily be immobilised with a minimum of interference of function, (2) the wrist has a wide range of well-balanced reciprocal movements, *i.e.* of flexion and extension, (3) wrist movement is one of the fastest and most accurate reciprocatory co-ordinations of the body.

2. THE FACTORS UNDER CONTROL.

Posture was regulated by seating the subjects on a heavy chair of the Zander type with adjustable seat and back rest. The right forearm, flexed horizontally, was supported from below and at the rear by a padded elbow rest and by two semi-circular clamps regulated from a slotted ring to hold the radius and ulna. These supports comprised a unit (Pl. I, fig. *a*) and were fully adjustable to support the forearm firmly in a natural position to the body.

The hand whose movement was to be registered was inserted with fingers fully extended into the padded aluminium mitt (fig. *b*) which extended well back over the metacarpals and was secured by two wire

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[1] "The Maximum Work and Mechanical Efficiency of Human Muscles, and their Most Economical Speed." *J. of Physiol.* LVI, Nos. 1 and 2, 1922.

Book No.........
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straps with screws. The mitt was attached to a strong aluminium frame carried on a steel axle in ground bearings. The mitt, the fore-arm fastenings and the subject's chair could be adjusted to bring the wrist joint in alignment with the axis of the registration unit. The axle of the latter carried four permanent fixtures (see fig. *b*): (1) a light tube containing a straight recording stylus which was projected into contact with the kymograph drum by a mild spring in the tube. (2) An indicator which showed to the subject his excursion of movement upon a protractor dial, the remaining equipment being screened from view by a curtain. (3) A friction disc, 5 by $\frac{3}{16}$ inches, with felted clamps on opposite sides which could be released or compressed by a screw to give a measurable resistance to the mobile unit. (4) An adjustable counter-weight. The mobile unit was made as compact and as light in weight as possible. It was supported upon the inner of two circular iron rings which permitted the mitt to be operated in a vertical or horizontal position. In order to overcome vibration arising from reciprocatory action, the registration unit had to be substantially constructed. Attachment of its supporting frame by a casting to the bed of an engine lathe gave satisfactory rigidity. A special kymograph driven by a constant speed motor and giving a paper speed of 70 cm. per sec. was built on this bed. Time was taken with a 50 D.V. electrical fork. Kymograph papers about 3 metres long were used which permitted consecutive registration for periods of from 3 to 4 seconds.

Tracings were taken of reciprocatory wrist flexion and extension[1], the movement being symmetrical about the neutral position of alignment of the hand with the forearm. The range of hand movement was placed under visual control of the subject by the indicator which reproduced on a dial the excursion of the hand and of the recording stylus. This type of control was adopted because of the general importance of eye-hand co-ordination and also in order to limit distraction by screening all equipment other than this visual stimulus.

Preliminary trials with the subjects were taken to determine the most suitable amplitude for investigation as judged by the combined criteria of speed and accuracy of movement. After trying ranges of 3, 5, 10 and 20 degrees, a standard amplitude of 7½ degrees was selected as a suitable optimum[2]. This angular excursion gave a lineal displacement of 34 mm. on the kymograph drum. A pair of tracer points were arranged to mark these limits on the graph in correspondence with the visual stimulus. A subject was not mechanically restricted from making a greater or smaller excursion than this standard stimulus prescribed, and there was regularly a certain amount of over-stroking and under-stroking, the range of amplitude being distributed more or less closely about the standard.

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[1] The terms flexion and extension are used throughout in the anatomical sense.

[2] As the amplitude was increased and the visual stimulus extended farther into the field of indirect vision, accuracy decreased; and also with very limited amplitudes the uniformity of excursion greatly decreased (reckoning the mean deviation as a percentage of the amplitude aimed at) with relatively little gain in speed. There was a certain amount of individual variation in respect of the optimum amplitude.

Thirty-seven adult subjects were used, including four women. Their muscular co-ordination, as judged by ordinary tests of steadiness, tapping and a hand dynamometer, appeared normal. The right wrist was used in all cases. Subjects were instructed to operate at their maximum of voluntary effort, aiming at both speed and accuracy. Preliminary training was given upon different occasions until the subjects were thoroughly accustomed to the combined kinaesthetic and visual control and until improvement by practice had practically ceased. For permanent graphic records the subject commenced action and upon a signal from him a sample of his accomplishment was taken over a period of about 3 seconds. Ample rest spells, with the supports loosened, were given between trials.

The muscles contributing to this oscillatory movement are numerous. The aim was not to differentiate or gauge their respective contributions but to examine the character of movement resulting from their integrated action. It may be assumed that the major flexors and extensors of the wrist would operate at full capacity. With the fingers held in full extension it is probable that the flexors and extensors of the fingers would also contribute to a minor extent to wrist movement[1]. The flexors of the fingers, however, would likely contribute more than would the finger extensors when the hand was fully open, whereas the reverse would be the case had the experiment been made with the fist closed. Whatever be the precise composition of the muscle action involved, its complexity is of interest in view of certain peculiar uniformities that apparently always obtain in the resultant movement of the wrist.

3. THE VARIABLES INVESTIGATED.

Under the above fixed conditions tracings were taken with the wrist in different positions and with different frictional loads. A further differentiation in each tracing was between flexion and extension curves; each uni-directional movement of the wrist is termed a 'stroke,' and reciprocating strokes are said to be in 'flexion phase' or in 'extension

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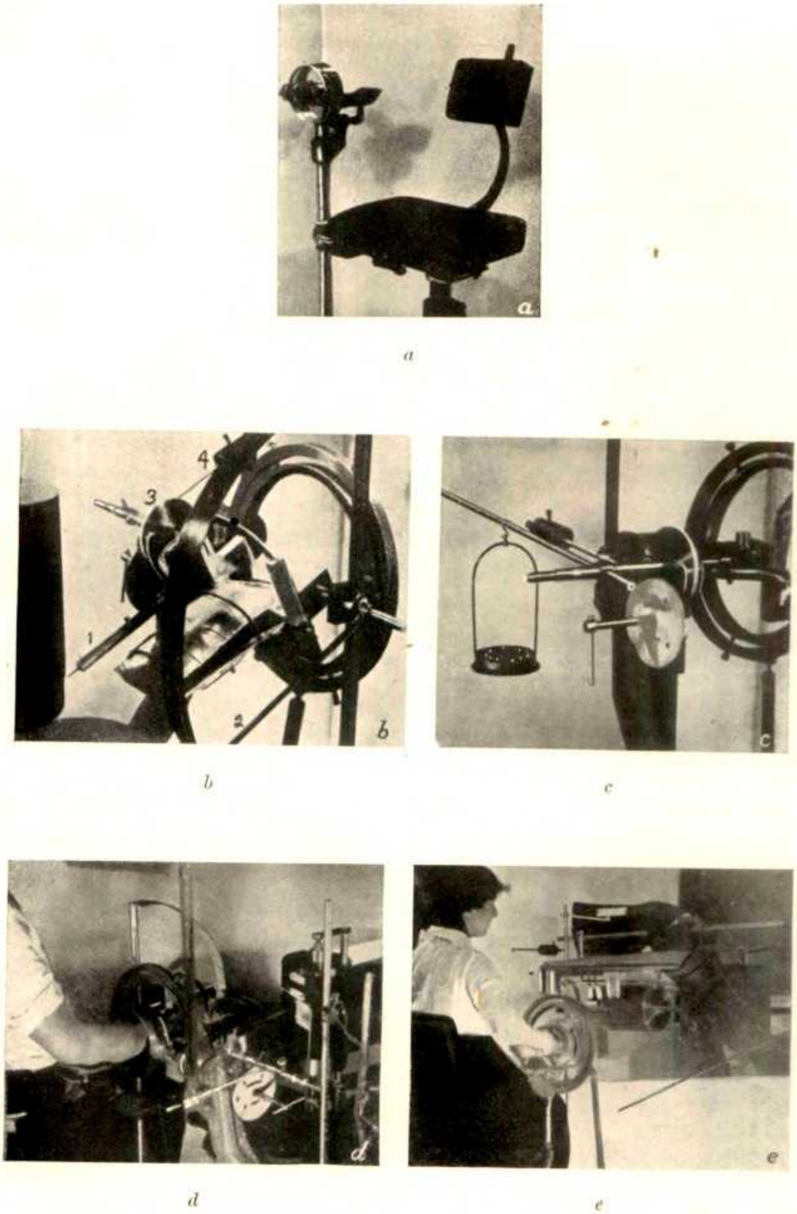
[1] Evidence of this is obtainable crudely by embedding tambours over selected tendons.



a

b

c

d

e

BOTT.—Some Characteristics of Reciprocal Wrist Action (pp. 1—24.)

phase.' The conditions brought under investigation with respect to their bearing upon wrist action were, therefore, those of position, load and phase.

The effect of position (*i.e.* of the degree of fore-arm rotation) is probably two-fold. First, it affects the mechanics of tendon connexion both in the degree of leverage given upon the mobile member and in the gravitational action of the latter. Secondly, in so far as changes of forearm position involve alterations in the distance between the forearm muscles and their respective insertions, the character of muscle action will be affected. If, for example, these changes are compensated by an appropriate lengthening or shortening of the respective muscles, then the position (in respect of forearm rotation) will be a factor in determining the working length (and possibly the tonus) of the muscle.

Three positions of rotation were selected, namely, with palm downward, inward and upward. The arrangement for the first or pronated position is shown in Pl. I, fig. *d*, for the second or mid-position in fig. *e*, the third or supinated position having the same arrangement as for pronation.

Considerable difficulty attaches to the assigning of a known load in reciprocal muscle action, particularly in respect of the change of phase. No satisfactory means of graduating a load during the progression and reversal of a stroke was found. As the object was to investigate the character of muscle action, not the maximum efficiency of muscles, the principle of constant loads was accepted. The administration of resistance by friction with four pads of piano felt each 1 inch diameter upon two hinged Y-shaped clamps compressed by a screw (fig. *c*) gave satisfactory results. The amount of load was measured and checked by a weight balance, three conditions of load being adopted as follows—

With the empty registration unit counterbalanced in a horizontal position and the resistance pads removed from contact with the disc, a weight equivalent to ·04 inch-pound sufficed to displace the registration unit slowly by about one-half the extent of a standard stroke. This amount of resistance (from the bearings of the unit) was scarcely perceptible to a subject and was accepted as a 'zero' or free load. Secondly, that compression of the pads which just permitted a similar displacement of the unit when a weight of 2½ lbs. was suspended 6 inches from its axis, fig. *c*, was accepted as a 'single' or medium load. Introspectively this load of 15 inch-pounds was very appreciable for adults, being about the maximum for a 6-year-old child. Thirdly, resistance equivalent in a similar way to 30 inch-pounds was taken as a 'double' or heavy load. This load was sufficient to tax the strength of the majority of the subjects. On test the mechanism was found to maintain these single and double loads without change after each of several tracings. There is, of course, the possibility that a frictional load so arranged would be momentarily augmented at the points of change of stroke phase. If this be the case there was no indication of the fact from inspection of the curves obtained, which were as uniform at those points as elsewhere. In the case of pronation and supination where gravity was a factor affecting the hand, an estimated allowance was made with the counterbalance to neutralise the weight of the subject's hand.

A subject made one tracing (which included both phases of strokes) with each of the above loads in each of the three positions. Tracings without a load gave on the average 10 strokes of each phase, those with a single load 8 and with a double load 6, for each of the 37 subjects. This amount of data was felt to be sufficient for statistical purposes. In order to check certain points which might depend upon individual variation, a far larger amount of data was secured with one male subject.

4. QUANTITATIVE RESULTS.

Certain general characteristics of the graphic data conditioned the mode of treatment. Variation of amplitude in these volitional movements introduced certain complications.

Fig. 1 shows practice strokes of two subjects taken in pronation without load and at slow kymographic speed. Inspection of all the data from this standpoint showed:—(1) There was greater variation in the amplitude than in the speed of stroking for each individual and also between individuals. (2) There was no discoverable regularity in the incidence of long and short strokes: they would appear singly or in groups and might come in quick succession or at considerable intervals. They appeared explosively rather than gradually and did not seem to compensate each other immediately. The excess or defect of amplitude characterized one extremity of the stroke as much as the other, irrespective of where the subject focussed his visual attention. Working at their maximum speed, subjects rarely had any accurate introspective impression of the degree of accuracy they had attained at a sitting, or at best they had only a total impression and none concerning individual strokes. Variation in amplitude cannot be attributed to voluntary adjustment from stroke to stroke, except in the rare instances when an extreme involuntary variation would provoke a conscious recovery. (3) There was sufficient variation in the length of the strokes to necessitate setting a limit for purposes of computation. A margin of 50 per cent. in excess or defect of the standard stroke of 34 mm. (*i.e.* a range of 51 to 17 mm.) was adopted. About 5 per cent. of data were disqualified by this criterion.

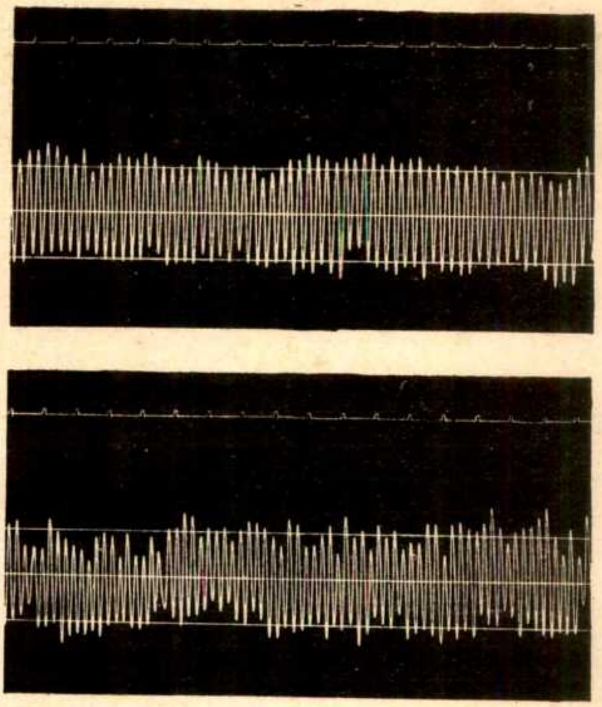


Fig. 1. Practice strokes.

Fig. 2 shows the type of curves obtained for pronation with the zero, single and double load, the time line and standard stroke being

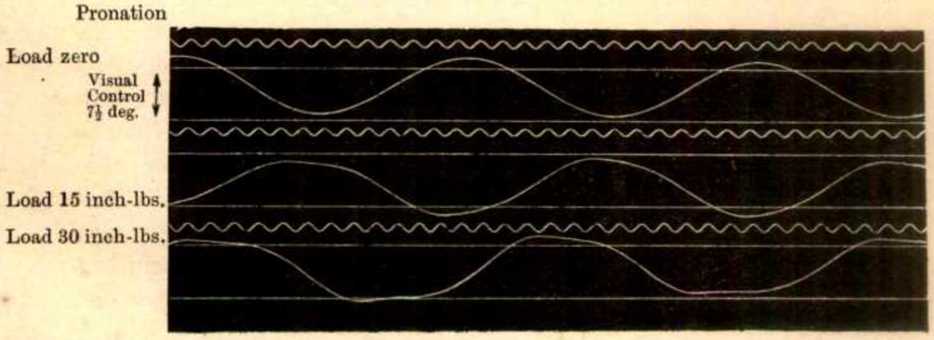


Fig. 2.

reproduced for each set of curves. The graphs were read as follows: Using a long straight edge parallel to the tracer lines, the point of farthest excursion of a stroke could be determined with respect to the time scale with an accuracy of about ·5$^\sigma$. Perpendiculars from these points to the time line were taken to divide the flexion from the extension phase. Three measurements were then made upon each phase of stroke viz. (*a*) the total amplitude in mm., (*b*) the total duration in 1/1000 seconds, (*c*) an analysis of the progression of a stroke in terms of its amplitude and duration. The form of progression was ascertained in terms of the ordinate altitude at the middle of each consecutive tenth of the stroke's duration as given lineally on the graph[1]. The duration of strokes being variable, these ordinate positions were found by use of a paper scale subdivided into 10 units each with a mid-point, the scale being slightly longer than the lineal duration of the stroke. By placing this scale at such an angle that its extremities coincided with the vertical end-lines of a stroke, and by reading perpendicularly from the mid-points on the scale to the curve, the required positions were obtained. By applying this principle of parallel section likewise to the amplitude (with a second paper scale of 100 mm.) each ordinate height could be read horizontally on to the latter scale directly as a percentage of the stroke amplitude. This procedure gave ten readings of progression each expressing the percentage of total altitude that had been attained (in ascending or descending order) at the mid-point of each successive tenth of duration of the stroke. Amplitude and duration each being variable, this relative measure of progression was adopted in order to be able to compute and plot these variables on a percentage basis for the entire data. With the use of specially adapted measuring instruments and a reading glass, the degree of precision in these readings was ·1 per cent. of amplitude for the eight central ordinates and about ·3 per cent. for each of the terminal ordinates—as judged by the measurements being reproducible by different persons.

A sample of readings for flexion and extension strokes in the pronated position, zero load, is shown in Table I. The ten columns at the left show the progression of each stroke in percentage of amplitude. The gross amplitude in mm. and the gross time as measured from the tracing are in the next two columns. The final column, computed from the two preceding, is the average rate of progression in terms of the number of thousands of seconds required per mm. of amplitude for each stroke. This will be termed the time-rate or *progression velocity* of the stroke. From such data the following points can be detected by inspection: (1) The form of progression for a given position, phase and load has a high degree of uniformity. (2) The amplitude has a wide variation above and below the standard stimulus of 34 mm. (3) The duration of strokes varies appreciably but relatively less than the amplitude and cannot be said to be even approximately proportional to the latter[1]. (4) Differences in amplitude correspond closely in an inverse sense with the progression velocity, *i.e.* the longer the stroke the shorter the time required per mm. of amplitude, or in general long strokes progress at a more rapid average rate than short ones. A part of the statistical problem was to determine the degree to which these relationships obtained in the data as a whole.

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[1] This assumed the time line (*i.e.* the speed of the kymograph) to be constant and where any irregularity was observed the data were omitted. As the average speed of the kymograph was 14 mm. per d.v. of the fork (20$^\sigma$), this assumption presented no practical difficulty.

TABLE I.

Name - C. R. M. Pos. Pronated Load Free

Flex. Amp. Str.	% of Amp. at successive 10ths of stroke time										Amp. in mm.	Time in σ	Aver. rate in σ per mm.
	1	2	3	4	5	6	7	8	9	10			
1	99·4	94·5	85·1	71·8	56·6	41·2	27·1	14·9	6·1	1·4	36·2	112	3·09
2	99·5	95·5	86·9	74·3	59·4	43·8	29·2	15·9	6·3	0·5	39·7	114·2	2·88
3	99·1	95·2	86·5	74·0	59·8	44·6	29·4	16·1	6·4	0·7	43·5	120·5	2·77
4	99·1	94·1	85·2	72·6	58·9	43·1	28·1	16·3	6·6	1·2	42·3	118	2·79
5	98·5	94·5	85·0	72·5	58·0	42·5	28·0	15·2	5·8	1·0	40·0	118	2·95
6	98·7	93·3	86·4	70·8	56·0	40·6	25·7	14·1	4·6	0·3	38 9	117·7	3 02
7	99·4	95·3	85·1	72·5	57·0	41·8	27·5	14·6	5·3	0·9	34·2	113·5	3·32
8	99·3	94·6	85·5	74·1	59·3	44·4	28·6	15·5	6·7	0 7	29·7	120	4·04
9	99·3	94·0	85·9	73·8	60·0	41·9	29·8	16·0	5·3	0·4	28·2	109	3·87
10	99·2	95·0	85·5	73·7	59·5	43·8	29·1	15·6	5·0	0·3	39·8	116·5	2·93
11	99·8	95 6	83·0	76·0	61·8	46·0	30·9	17·6	7·1	0·9	45·0	119	2·65
Ext.													
1	1·0	5·4	15·2	25·8	39·9	55·6	69·6	84·4	94·1	98·7	38·8	115	2·97
2	1·0	4·9	13·8	26·4	40·8	55·9	70·8	84·1	93·1	99·0	39·0	113·5	2·91
3	0·5	4·6	13·2	25·5	40·8	57·0	77·5	84·8	94·3	99·1	43·9	117·5	2·68
4	0·8	4·9	13·3	25·5	40·1	55·6	71·0	84·0	93·7	99·5	36·9	114·5	3·10
5	1·3	5·5	14·4	26·5	40·6	56·5	71·6	85·2	94·3	99·2	39·7	116·8	2·94
6	0·3	4·5	13·4	25·1	39·7	56·7	72·4	85·5	94·4	98·9	35·8	112·5	3·14
7	1·3	6·6	15·6	28·7	43·5	59·4	74·4	86·3	95·0	99·4	32·0	119·5	3·74
8	1·0	5·6	13·9	25·8	39·7	56·0	72·6	84·8	94·5	99·3	30·2	111	3·68
9	0·8	4·9	14·1	26·9	42·1	58·5	73·2	85·6	94·8	98·0	36·8	114·5	3·11
10	0·5	4·5	14·4	26·3	41·6	57·2	73·2	84·5	94·0	98·6	41·8	120	2·87

With the amplitude and duration of strokes varying independently, any procedure for computing the average amplitude of a series of strokes and the best average duration corresponding thereto for that series would be somewhat arbitrary. It is desirable to do this, however, in order to compare, for example, the relative rate of incidence (number of strokes per second) for each phase with different positions and different loads. The following plan of averaging was adopted. (1) From the total time and amplitude of a stroke the number of time units per unit of amplitude was obtained, giving a rate of progression velocity in σ per mm. as indicated in Table I. (2) These rates were then averaged for the strokes under consideration, giving an average progression velocity for the series. (3) The amplitudes of the strokes for that series were also directly averaged. (4) The product of (2) and (3) above was then taken as an average duration value of the strokes in that series. (5) The reciprocal of this duration value was accepted as representing the incidence of such strokes per second.

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[1] Compare, for example, the 3rd, 8th and 9th flexion strokes or the 7th, 8th and 10th extension strokes, Table I.

Findings dependent upon position, phase and load.

Three points which appear to be conditioned by these factors (other things being equal), are reported for the combined data of 37 subjects, viz. the accuracy of strokes, the rate of incidence and the form of progression.

Diagram 1 summarises the data upon *accuracy*. The ordinate scale is in minutes of arc of wrist movement, the standard stroke being the distance from the base line to the middle horizontal line (450 min.), the upper and lowermost horizontal lines being the limits outside which long and short strokes respectively were rejected. The datum points show the average amplitude of strokes for the three positions with each phase and load, and the perpendicular lines at the base are the respective mean deviations of stroke length in minutes of arc—both the spread and the central tendency being significant as regards accuracy. Each datum point represents from 200 to 400 strokes.

The main indications from Diagram 1 are:—(1) A moderate load gives greater accuracy (approximation of the central tendency to the standard) than either no load or a heavy load, but the mean deviation tends to increase slightly as the load is increased from zero. (2) The mid-position tends regularly to under-stroking which increases slightly with the load. Both pronation and supination tend on the whole toward over-stroking, the standard stroke being more closely approximated with a load than without. Supination regularly shows the greatest spread. (3) There is no significant difference in accuracy between the flexion and extension phase owing to the conditions of control.

Next, the *rate of incidence* (*i.e.* the number of strokes per second, computed in the way indicated) shows definite dependence upon the variables investigated, particularly upon the load (see Diagram 2):

(1) Without a load there is practically no discrepancy in rate between the flexion and extension phases in any position. There is, however, a difference in rate dependent upon position; the maximum rate is for mid-position, viz. 9·37 strokes per sec., the pronation rate being 4½ per cent. slower and the supination rate 12 per cent. slower than that for mid-position.

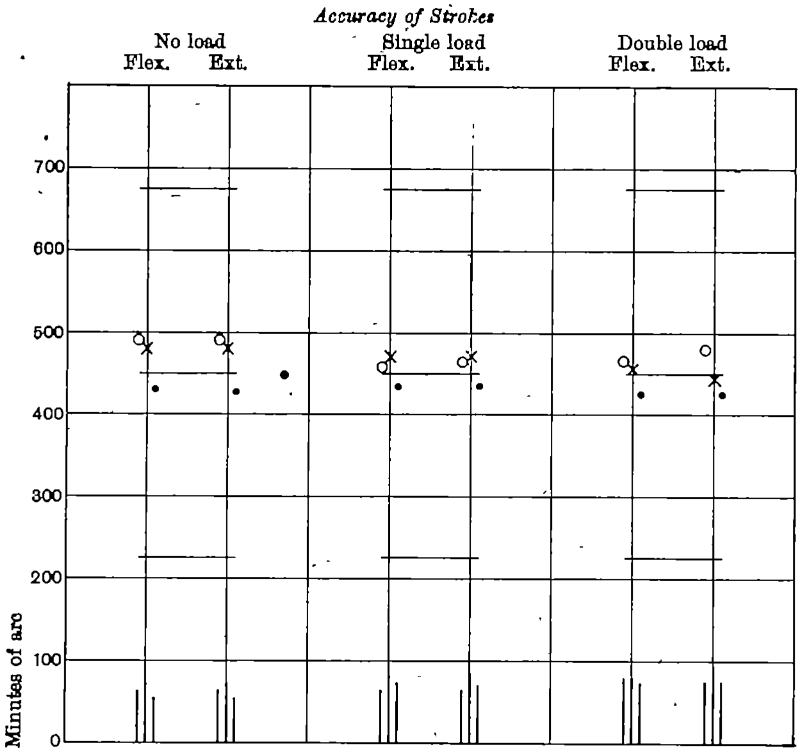

Diagram 1. Standard stroke 450 min. Upper limit 675. Lower limit 225. Average amplitude: o pronation, × supination, • mid position. Mean deviations are shown from the abscissa.

(2) With a load the rate is lowered, the average decrease (for both phases and all positions combined) being inversely proportional to the load—the decrease from the average rate without a load (8·86 strokes per second) being 17·3 per cent. for the single load and 34·1 per cent. for the double. As the latter load was nearly at the limit of immobility for several of the subjects, the data suggest that the decline in rate of

incidence in respect of load follows a straight line relationship to this point or somewhat beyond it and would then fall steeply to zero. This indication would require verification with a number of intermediary and excessive loads, but within the limits and under the conditions of the data at hand the relationship as noted constitutes a law of voluntary muscle action, viz. that the rate of reciprocating movement under maximum effort decreases by an equal amount for equal increments of load.

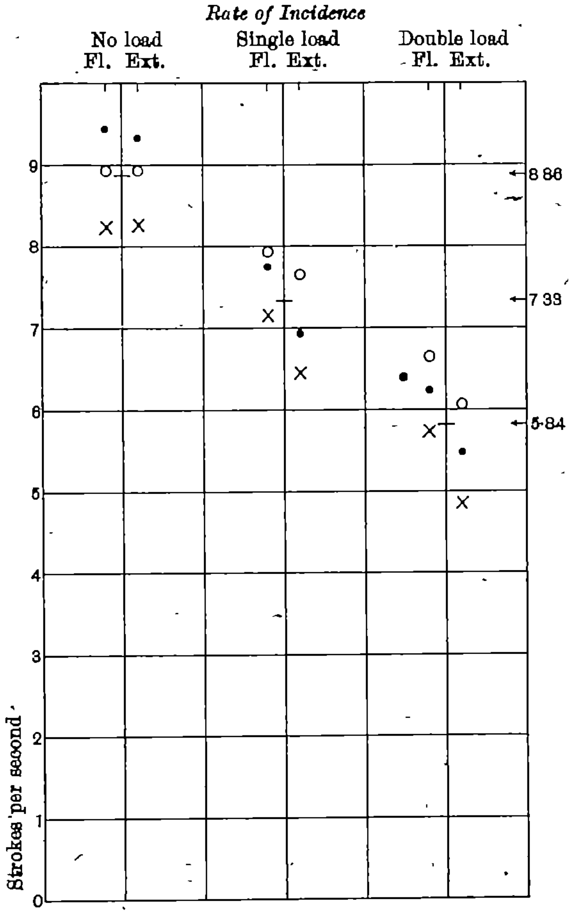

Diagram 2. • Mid position, ○ Pronation, × Supination,
– Average for both phases and three position.

(3) The rate with a load as compared with the rate of free motion exhibits differences dependent upon position and phase.

(*a*) The most favourable position for fast reciprocating motion (of the wrist) with a load is full pronation, the mid-position being less favourable while full supination remains the least favourable here as it is in free motion.

(*b*) With a load the rate of incidence is regularly greater for the flexion phase than for extension[1], and the amount of this discrepancy differs with the position and the load. In pronation and in supination the superiority of flexion grows greater as the load is increased—more rapidly in the case of supination; in the mid-position the difference in favour of flexion is somewhat greater for a medium than for a heavy load. These detailed variations of rate are compensatory when considered as a total resultant[2] with a given load, as expressed in (2) above.

(*c*) Comparing the rates for the respective positions with such a resultant rate (at a given load), pronation is superior in both phases, supination is inferior in both phases, while the mid-position is superior for flexion and inferior for extension.

Finally, the *form of progression* depends upon position, load and phase but not as independent variables. The form of stroke progression for each relationship of these variables was expressed as a percentage of amplitude to percentage of stroke time, ten readings being taken as indicated (*supra* p. 8). The number of strokes employed to determine the characteristic progression for each relationship ranged from over 400 to about 200. The readings for the 37 subjects showed close uniformity, the greatest mean deviation in pronation with no load, for example, being under 1·7 per cent. of the total stroke amplitude. With a single and particularly with a double load the mean deviation of progression readings for this group of subjects rose above this amount[3]; but as this was found not to be the case with one subject when tested by numerous readings, it may be said that there is a high degree of constancy in progression for each subject with a given load. An average progression curve for several subjects is therefore significant. In Diagram 3 (and 5) progression curves are plotted as dependent upon the variables under consideration. The scales are arbitrary, representing percentage of amplitude and of duration; the progression of a stroke from its inception (whatever the phase) is plotted from the point of origin.

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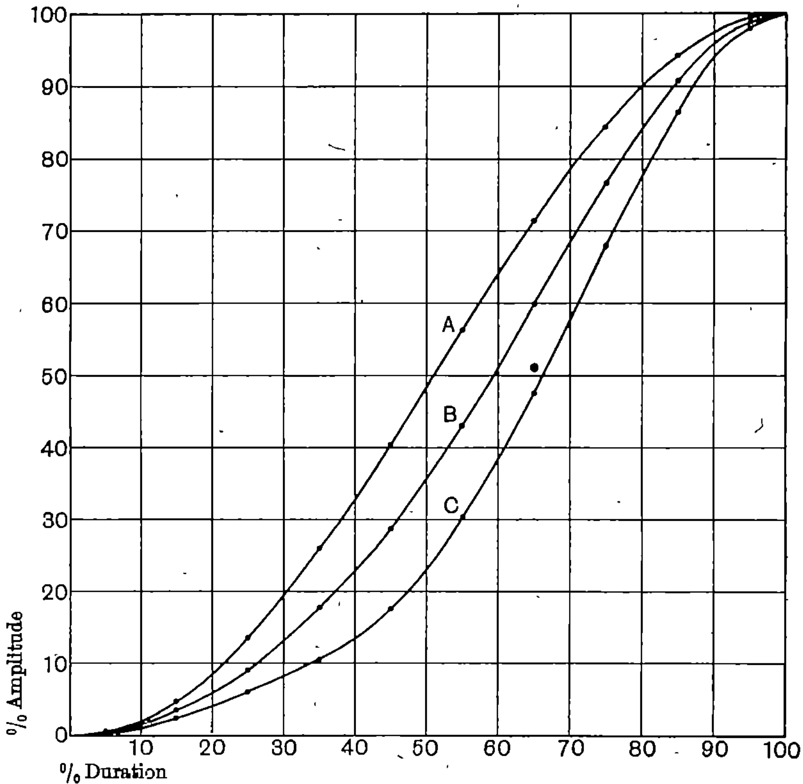
[1] The advantage of the finger flexors when the hand is kept fully open may be partly responsible for this, *supra* p. 4.

[2] The significance of considering the resultant or averaged rate is that under ordinary conditions of manual manipulation change of forearm position frequently occurs to a greater or less extent along with wrist movement.

[3] This was due apparently to the wide difference in gross muscle strength of the subjects.

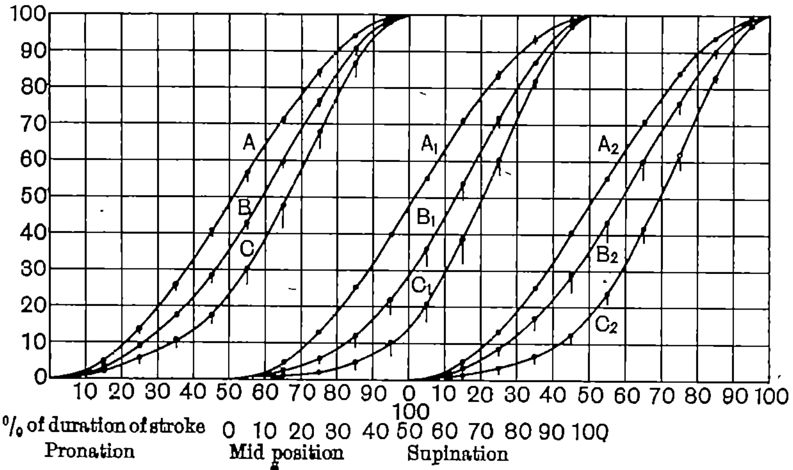
In Diagram 3, curves *A*, *B* and *C* show the progression of pronated flexion strokes with zero load, a single and a double load respectively. Curve *A* is seen to be nearly symmetrical although not exactly so, *e.g.*



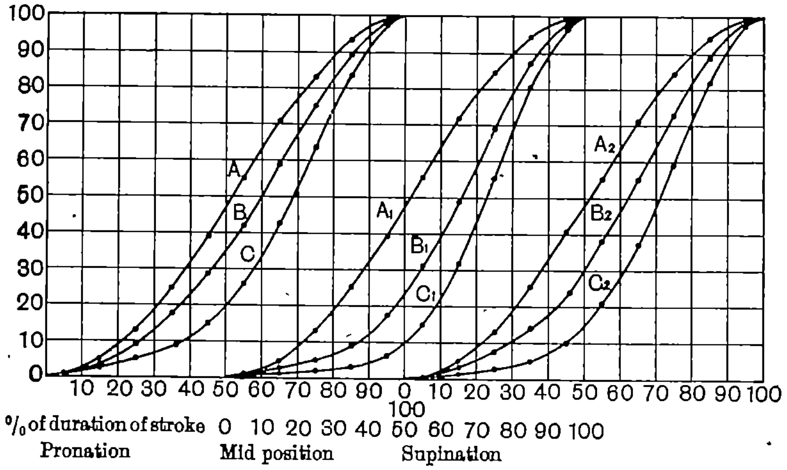
Load. No strokes.
A. ·04 inch lbs. (zero) 361. B. 15 inch lbs. 306. C. 30 inch lbs. 218.

Diagram 3. Progression of pronated flexion strokes with no load—single medium load—double load. Ordinate shows % of amplitude; abscissa % of duration.

at 50 per cent. of stroke time 48 per cent. of amplitude was covered, at 25 per cent. time 13·7 per cent., at 75 per cent. time 84·3 per cent. etc. This particular form of curve for zero load, however, is significant in as much as it holds uniformly (within 1 per cent.) for the mid-position and for supination as well as for pronation, and for extension as well as for flexion (Diagram 4). Again, from Diagram 3 a relationship between the curves *A*, *B* and *C* that may be seen by inspection is that the difference in percentage of amplitude between the *A* and *B* curve is equal to that between the *B* and *C* curve at each successive point of the stroke time[1].



FLEXION. A. No load. B. Single load. C. Double load.



EXTENSION. A. No load. B. Single load. C. Double load.

Diagram 4.

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[1] For the most part this relationship (in pronated flexion) is one of nearly exact equality; it varies therefrom by an amount slightly exceeding 1 per cent. of the stroke amplitude only in the second and third tenth of stroke time. The relationship is such that the progression curve for a double load, for instance, could be deduced from those for a zero and a single load with almost complete accuracy.

Or generally, in pronated flexion the amount of amplitude loss in progression is proportional to the load[1].

Diagram 4 shows the complete data of progression curves for the three forearm positions and both phases of stroke. The perpendicular lines from the datum points of the flexion curves indicate the amount of displacement therefrom (if any) for the extension curve under like conditions of position and load. The main uniformities in these progression data are two:—(1) The A curve (zero load) remains practically unchanged, *i.e.* is independent of position or phase. (2) The B curve (single load) is identical in but three cases, viz. pronated flexion, supinated flexion and pronated extension. It is therefore partially dependent on both position and phase. The C curve (double load), on the contrary, has a different form in every case and is thus wholly dependent on position and phase.

Considering all of the flexion curves, however, there is a fairly simple relationship of progression in terms of amplitude defect, which obtains with an accuracy of within 1·5 per cent. of amplitude. Expressing in pronated flexion the amplitude defect between A and B (and likewise between B and C) as X, then the corresponding amplitude defects in flexion at the mid-position are $\frac{3}{2} X$ and $\frac{9}{8} X$, while in the supinated position they are X and $\frac{3}{2} X$. The displacement in the extension curves, however, is such that proportionality of amplitude defect does not hold in the progression data in any simple way. There is, of course, no justification for accepting the above relationship of the A and B curves of pronated flexion as a standard of comparison except the fact that the former curve is constant for each position and phase and the latter in three of the cases.

5. OTHER RESULTS.

In addition to the characteristics of stroke accuracy, rate of incidence and form of progression, which have been presented as depending upon position, phase and load, there are other features of wrist action that are very definite and consistent. Two of these which are of special interest may be mentioned, one pertaining to the variability of amplitude, the other to the sequential relationship of strokes in opposite phase.

One of the outstanding points of the data is the erratic over-stroking and under-stroking by a subject when trying to reproduce a given standard stroke. In order to understand this persistent feature of voluntary muscle action as actually recorded, the real difference between long and short strokes must first be determined—for amplitude as such turns out to be merely an effect.

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[1] The amplitude defect with a load as shown in the curves of Diagram 3 increases from zero as the stroke proceeds, reaching a maximum in the sixth tenth of the stroke time, and then decreases as the stroke is completed. If this amplitude defect with a single load (*i.e.* between the curves A and B) be represented as X (the value of X being dependent upon the extent of progression), then the amplitude defect between B and C is likewise X.

The other problem, which probably underlies the question of amplitude, is the effect which a preceding stroke has upon its successor. Reversal of stroke phase in reciprocal action involves, for one thing, alternation in the innervation of the complex musculature involved, but how and when does this alternation occur in the phase cycle? What do graphic data indicate regarding the nature of phase change? Only the first of these problems, the matter of amplitude variation, will be dealt with in the remainder of this paper.

The nature of long and short strokes.

In considering the measurements of each recorded stroke (Table I, p. 9) it was noted (1) that strokes varied considerably in amplitude, (2) that the stroke time varied too but not strictly in proportion to the amplitude, and (3) that the average rate of progression tended to vary inversely as the amplitude, *i.e.* that the time required per mm. of amplitude was regularly greater in short strokes than in long ones[1]. A first step was to determine these relationships quantitatively, *i.e.* the connexion between the amplitude of a stroke and its progression velocity and also beween the amplitude and the total duration. This was done by making scatter diagrams of strokes of all subjects taken under like conditions of position, phase and load. The result for pronated flexion with no load is in Table II. The ordinates show amplitude in 2 mm. intervals, the abscissa in one diagram being the progression velocity in intervals of $\cdot 1^{\sigma}$ and in the other the total stroke time in intervals of 2^{σ}.

The correlation ratios for these curvilinear distributions[2] are + ·80 and + ·54 respectively. That the degree of relationship expressed by these ratios has considerable significance is shown by the fact that values nearly identical with these are obtained from the data for each

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[1] This time-rate per mm. of amplitude was termed the *progression velocity* for the purpose in hand. Its numerical value (in time units) is always an average for the entire stroke computed from the total amplitude and duration.

[2] Pearson's formula for non-linear distributions was used,

$$\eta = \frac{\Sigma}{\sigma_y} = \frac{\sqrt{\frac{S[n_x(y_x - y)^2]}{N}}}{\sigma_y}.$$

TABLE II. *Correlation of amplitude with average rate of progression—Pronation flexion, no load.*

Amplitude in mm.	1000ths sec. per mm. of amplitude																																				
	2·0	2·1	2·2	2·3	2·4	2·5	2·6	2·7	2·8	2·9	3·0	3·1	3·2	3·3	3·4	3·5	3·6	3·7	3·8	3·9	4·0	4·1	4·2	4·3	4·4	4·5	4·6	4·7	4·8	4·9	5·0	5·1	5·2	5·3	5·4	5·5	
17·5 18·5 19·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1
19·5 20·5 21·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	1	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	3
21·5 22·5 23·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
23·5 24·5 25·4	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.	.	2	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	2	.	7
25·5 26·5 27·4	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1	1	.	.	1	1	.	.	1	.	.	.	.	.	1	.	.	.	.	.	.	.	7
27·5 28·5 29·4	.	.	.	.	.	.	.	.	.	2	1	.	.	1	.	5	.	.	1	.	.	.	.	.	.	.	1	.	1	1	.	.	.	.	.	.	13
29·5 30·5 31·4	.	.	.	.	.	.	.	.	3	1	1	3	2	2	4	1	1	.	.	.	1	.	2	2	.	1	.	.	.	.	.	.	.	.	.	.	24
31·5 32·5 33·4	.	.	.	.	.	.	1	2	4	3	1	2	5	8	1	.	.	1	1	3	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	33
33·5 34·5 35·4	.	.	.	.	.	1	1	6	2	3	4	1	6	5	1	2	4	1	3	.	2	2	.	1	.	.	.	.	.	.	.	.	.	.	.	.	45
35·5 36·5 37·4	.	1	.	.	.	3	10	7	7	5	4	4	4	1	2	3	4	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	56
37·5 38·5 39·4	.	.	.	.	2	4	13	7	3	2	4	1	2	2	6	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	48
39·5 40·5 41·4	2	.	1	5	9	7	3	3	4	4	2	1	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	42
41·5 42·5 43·4	.	.	1	5	6	11	2	3	1	3	.	.	1	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	34
43·5 44·5 45·4	2	1	7	2	.	1	3	1	2	1	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	21
45·5 46·5 47·4	1	2	5	1	.	.	1	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	11
47·5 48·5 49·4	1	1	.	2	2	2	2	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	11
49·5 50·5 51·4	2	1	.	1	.	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	6
	8	6	14	16	19	31	36	29	27	25	18	12	21	23	15	15	9	5	6	4	5	2	3	3	0	2	1	1	2	2	0	0	0	0	2	1	363

Correlation ratio +·80. p.e. ±·01.

Correlation of amplitude with duration of stroke—Pronation flexion, no load.

Amplitude in mm.	Duration in 1000ths sec																																				
	79	81	83	85	87	89	91	93	95	97	99	101	103	105	10	109	111	113	115	117	119	121	123	125	127	129	131	133	135	137	139	141	143	145	147	149	
17·5 18·5 19·4	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
19·5 20·5 21·4	1	1	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	3
21·5 22·5 23·4	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1
23·5 24·5 25·4	.	.	1	1	.	.	1	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	1	.	2	.	.	.	.	.	.	.	7
25·5 26·5 27·4	.	.	.	.	.	.	2	.	2	.	.	.	1	1	.	.	.	.	1	.	.	.	.	.	1	.	.	.	.	.	.	.	.	.	.	.	8
27·5 28·5 29·4	.	.	.	.	3	.	.	.	.	1	2	.	1	2	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	1	.	.	.	12
29·5 30·5 31·4	.	.	.	1	1	2	1	.	2	2	.	1	2	4	2	.	.	1	.	1	.	1	.	.	.	3	.	.	1	.	1	.	.	.	.	.	26
31·5 32·5 33·4	.	.	.	.	.	3	.	1	3	3	1	.	3	2	6	1	4	.	.	.	.	.	1	.	.	1	1	1	.	.	.	.	.	.	1	.	32
33·5 34·5 35·4	.	.	.	.	.	1	1	.	3	2	1	3	1	2	1	1	3	3	3	1	4	.	.	1	2	2	4	.	.	1	.	1	1	1	.	1	44
35·5 36·5 37·4	.	1	.	.	.	.	1	1	4	4	7	5	2	3	4	3	3	2	1	3	.	1	1	1	1	3	1	2	1	.	.	.	.	.	1	.	56
37·5 38·5 39·4	.	.	.	.	.	.	.	.	1	1	4	8	4	6	2	1	.	1	2	1	1	2	.	1	2	1	2	2	4	.	.	.	.	.	.	.	46
39·5 40·5 41·4	.	.	1	.	1	.	1	1	2	2	7	3	5	3	1	1	2	1	2	1	1	1	2	1	1	1	.	.	.	.	.	.	.	.	.	.	41
41·5 42·5 43·4	.	.	.	.	.	.	.	.	1	1	1	1	4	2	4	6	3	2	.	1	3	.	.	3	.	.	.	.	.	.	1	1	.	.	.	.	34
43·5 44·5 45·4	.	.	.	.	.	.	.	1	1	1	2	5	2	.	.	.	.	1	.	.	1	2	1	1	.	1	.	1	.	1	.	.	.	.	.	.	21
45·5 46·5 47·4	.	.	.	.	.	.	.	.	.	2	.	.	3	2	1	1	.	.	.	.	.	.	1	.	.	.	.	1	.	.	.	.	.	.	.	.	11
47·5 48·5 49·4	.	.	.	.	.	.	.	.	.	.	.	.	1	.	1	.	.	1	2	.	1	1	2	.	.	.	.	1	.	.	.	.	1	.	.	.	11
49·5 50·5 51·4	.	.	.	.	.	.	.	.	.	.	.	.	1	2	.	.	.	.	.	1	.	.	.	1	.	1	.	.	.	.	.	.	.	.	.	.	6
	2	2	2	2	5	6	7	4	19	19	25	26	31	30	22	15	16	12	12	9	11	8	8	9	7	13	9	8	8	2	3	2	3	1	2	1	360

Correlation ratio +·54. p.e. ±·025.

phase in all three positions with no load[1]. Furthermore the closeness of the actual correlation is here somewhat masked by personal differences in the many subjects, the results for each individual falling within limited blocks of the diagrams. For this reason and because this fictitious scattering (as between individuals) was still more conspicuous in the case of loaded strokes, the point was examined with three sets of data from one additional subject. These were with the three loads taken in the pronated position only. The distributions obtained with the medium load are illustrated in Table III.

The results for this subject as well as those mentioned for the 37 subjects were:

For 37 subjects:

			Amplitude varies *inversely* as the progression velocity			Amplitude varies *directly* as the total duration		
Position	Load	Phase	Ampl. of strokes	Correl. ratio	p.e.	Ampl. of strokes	Correl. ratio	p.e.
Pronation	Zero	Flexion	363	+*·80*	±·01	360	+*·54*	±·025
”	”	Extension	356	+*·84*	±·01	374	+*·43*	±·028
Mid-position	”	Flexion	419	+*·81*	±·011	419	+*·48*	±·025
”	”	Extension	424	+*·80*	±·012	424	+*·47*	±·026
Supination	”	Flexion	345	+*·82*	±·012	345	+*·47*	±·008
”	”	Extension	354	+*·82*	±·012	354	+*·54*	±·025
For one subject:								
Pronation	Zero	Flexion	117	+*·98*	±·003	117	+*·65*	±·035
”	”	Extension	115	+*·94*	±·007	115	+*·68*	±·033
”	Single	Flexion	207	+*·96*	±·003	207	+*·59*	±·03
”	”	Extension	209	+*·96*	±·004	209	+*·65*	±·027
”	Double	Flexion	128	+*·94*	±·007	128	+*·58*	±·039
”	”	Extension	134	+*·92*	±·012	134	+*·52*	±·043

From these data it appears:—(1) That the amplitude of a stroke has consistently a very precise (inverse) relation to its progression velocity and also a constant though somewhat looser (direct) relation to the total stroke time. (2) That these relations are in no way dependent upon the position, the phase or the load of the stroke. The fact is, therefore, that short strokes have a high progression velocity, *i.e.* they take on the average several σ per mm. while long strokes have a correspondingly low time-rate, this tendency obtaining to the extent denoted by a correlation upwards of + ·9. Thus short strokes are those which progress slowly while long strokes travel rapidly, the speed being the reason why a particular stroke turns out short or long as the case may be[2].

This fact of relationship may be otherwise stated to the effect that

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[1] The average ratio for amplitude to progression velocity in these six distributions was +·82 with an M.D. of 1·5, and for amplitude to total time was +·49 with an M.D. of 3·5.

[2] The proper problem is then not the amplitude (absolute or relative) of a stroke, this being an effect; the question is what conditions determine a stroke's characteristic velocity.

TABLE III. *Correlation of amplitude with average rate of progression.*

Pronation flexion. Medium load. One subject. 207 strokes.

Amplitude in mm	1000ths sec. per mm. of amplitude																																	
	1·9	2·0	2·1	2·2	2·3	2·4	2·5	2·6	2·7	2·8	2·9	3·0	3·1	3·2	3·3	3·4	3·5	3·6	3·7	3·8	3·9	4·0	4·1	4·2	4·3	4·4	4·5	4·6	4·7	4·8	4·9	5·0	5·1	
17·5 18·5 19·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	.	.	.	.	.	.	.	1	2
19·5 20·5 21·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	1	.	1	.	1	2	1	2	.	.	1	.	1	.	11
21·5 22·5 23·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1	2	2	.	1	.	.	1	.	.	.	.	.	.	.	.	8
23·5 24·5 25·4	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1	1	3	1	4	1	1	1	.	.	.	.	.	.	.	.	.	.	.	14
25·5 26·5 27·4	.	.	.	.	.	.	.	.	.	.	.	.	.	1	2	4	4	1	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	13
27·5 28·5 29·4	.	.	.	.	.	.	.	.	.	.	1	2	4	3	1	1	.	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	14
29·5 30·5 31·4	.	.	.	.	.	.	.	.	.	3	4	4	1	3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	15
31·5 32·5 33·4	.	.	.	.	.	.	.	3	3	6	4	3	3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	22
33·5 34·5 35·4	.	.	.	.	.	2	3	5	4	6	5	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	26
35·5 36·5 37·4	.	.	.	1	.	2	.	7	3	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	15
37·5 38·5 39·4	.	.	.	2	1	5	3	3	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	15
39·5 40·5 41·4	.	.	.	2	6	8	3	.	3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	22
41·5 42·5 43·4	.	1	1	2	5	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	11
43·5 44·5 45·4	.	1	5	.	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	7
45·5 46·5 47·4	.	2	5	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	7
47·5 48·5 49·4	.	.	2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2
49·5 50·5 51·4	2	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	3
	2	5	13	7	13	19	9	18	14	17	14	10	8	8	4	6	8	6	6	4	2	3	0	2	3	1	2	0	0	1	0	1	1	207

Correlation ratio +·96. p.e. ±·003.

Correlation of amplitude with duration of loaded strokes.

Amplitude in mm.	Duration in 1000ths sec.																			
	75	77	79	81	83	85	87	89	91	93	95	97	99	101	103	105	107	109	111	
17·5 18·5 19·4	.	.	.	1	.	.	1	.	.	.	.	.	.	.	.	.	.	.	.	2
19·5 20·5 21·4	1	.	1	.	1	2	1	.	2	1	.	1	.	.	1	.	.	.	.	11
21·5 22·5 23·4	.	1	1	.	1	2	.	1	.	1	1	.	.	.	.	.	.	.	.	8
23·5 24·5 25·4	.	.	.	2	1	2	3	2	2	1	.	.	.	1	.	.	.	.	.	14
25·5 26·5 27·4	.	.	.	.	.	1	2	3	4	1	1	.	1	.	.	.	.	.	.	13
27·5 28·5 29·4	.	.	.	.	.	1	3	2	5	.	1	.	.	2	.	.	.	.	.	14
29·5 30·5 31·4	.	.	.	.	.	1	1	5	3	2	1	.	1	1	.	.	.	.	.	15
31·5 32·5 33·4	.	.	.	.	2	.	3	1	3	1	7	1	.	2	2	.	.	.	.	22
33·5 34·5 35·4	.	.	.	.	.	2	3	3	2	2	3	5	2	2	2	.	.	.	.	26
35·5 36·5 37·4	.	.	.	.	1	.	1	.	1	1	2	4	2	2	1	.	.	.	.	15
37·5 38·5 39·4	.	.	.	.	.	.	2	1	1	2	1	4	1	3	.	.	.	.	.	15
39·5 40·5 41·4	.	.	.	.	.	.	.	.	4	1	4	3	5	1	1	.	1	.	2	22
41·5 42·5 43·4	.	.	.	.	.	.	.	2	.	.	2	1	2	2	1	1	.	.	.	11
43·5 44·5 45·4	.	.	.	.	.	.	.	1	.	2	1	2	.	.	1	.	.	.	.	7
45·5 46·5 47·4	.	.	.	.	.	.	.	.	.	1	.	2	1	3	.	.	.	.	.	7
47·5 48·5 59·4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	2	.	.	.	.	2
49·5 50·5 51·4	.	.	.	.	.	.	.	.	.	.	.	1	.	1	1	.	.	.	.	3
	1	1	2	3	6	11	20	21	27	16	24	24	15	20	12	1	1	0	2	207

Correlation ratio +·59. p.e. ±·03.

reciprocating strokes, however irregular in amplitude, tend to have about the same duration (constant periodicity), short ones taking longer time per unit of amplitude and long ones less. Nevertheless this tendency is not completely realised, as is shown by the further fact that amplitude varies directly as the total stroke time with a positive correlation of about ·6. To this latter extent, it must be accepted that longer strokes always occupy actually longer time and shorter strokes less time. Moreover this degree of failure to achieve exact periodicity is found in all of the data with the same consistency that is enjoyed by the high correlation between amplitude and progression velocity.

Whatever be the connexion between this strong tendency toward periodicity and the weaker tendency away from it, the data permit the problem of over-stroking and under-stroking to be driven back at least one stage further. Although the amplitude of strokes correlates very closely with their progression velocities, the measure of the latter which gave this correlation was merely an average for the whole stroke. It does not reveal, for example, whether the unique slowness of a short stroke was chiefly in the late or early part of the stroke or whether it was distributed in some uniform way throughout the whole extent of each short stroke, and similarly of the rapidity that characterises long strokes.

This question however, permitted of a precise answer from the data, because the form of progression of every stroke had already been ascertained, as indicated in Table I. The following procedure was adopted in order to compare the form of progression of slow and fast (*i.e.* short and long) strokes respectively. From a distribution of strokes (such as in Table II) three groups were selected on the basis of amplitude length, viz. a third which were the shortest, another third of medium length and the remaining third which were long. The progression curve for each of these groups was then found, the average curve for the combined data being already known as presented, for example, in curve *A* of Diagram 3. Comparison of these three curves of selected data and the unselected curve of the distribution would then reveal any differences in the form of progression consequent upon differences in stroke amplitude.

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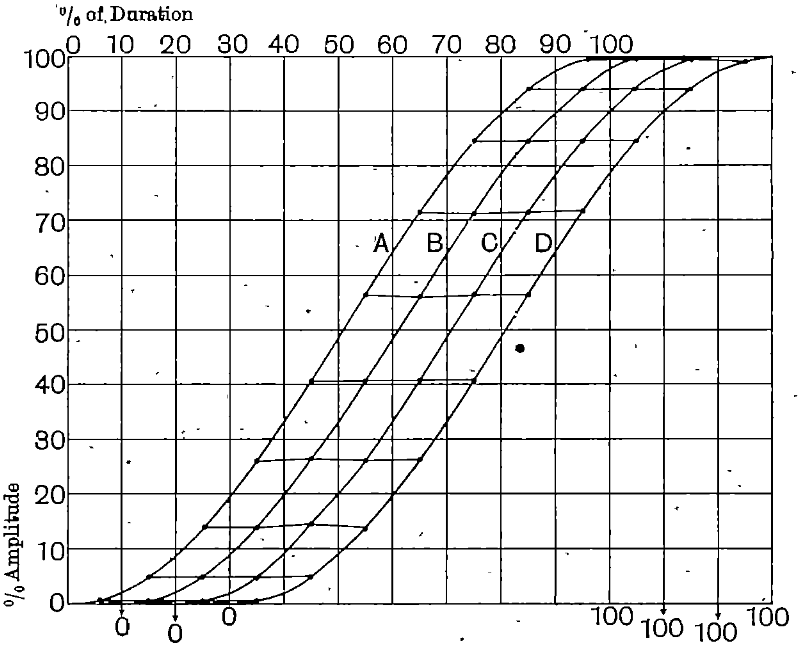
Diagram 5 shows the result for 360 pronated flexion strokes by all the subjects with no load. Curve *A* is the average progression for all of these strokes, *B* for 100 short strokes, *C* for 108 of medium length and *D* for 111 long strokes. For purposes of comparison the curves are displaced ten units on the abscissa instead of being superimposed and the corresponding datum points are connected horizontally[1]. From this distribution it is seen that short strokes have the same form of progression (percentage of amplitude to percentage of time) as those of medium or excessive length. This characteristic was also examined for strokes with zero load in each position and phase (*i.e.* for the distributions giving each of the *A* curves in Diagram 4 and was found to hold



A. 360 unselected pronated flexion strokes (ampl. 17–51 mm.)
B. 100 short ,, ,, ,, (ampl. 17–34 mm.)
C. 108 medium ,, ,, ,, (ampl. 35–39 mm.)
D. 111 long ,, ,, ,, (ampl. 40–51 mm)

Diagram 5. Progression of short and long strokes with zero load.

in all cases with about the same accuracy that is shown in Diagram 5. And this property likewise holds for a single or a double load—the plotted result for the former (from one subject with pronated flexion) being shown in Diagram 6.

The relationship here indicated constitutes a law of Identical Progression. It was shown previously that the type of stroke progression depended upon the position, load and phase (Diagram 4). The present relationship is that all strokes under like conditions of these variables have an identical form of progression irrespective of the amplitude of the stroke. From this and from the two additional facts, viz. that the amplitude varies widely and that strokes progress at a rate that is inversely proportional to their amplitude, it follows that these differences between the progression rates of strokes apply in each case uniformly through the extent of a stroke. Thus a stroke is short not simply because it is slow in an average sense but because it *started* slow, its curve of progression thereafter being the same as that of a long fast stroke or any other[1]. In this respect long and short strokes are pendular in action

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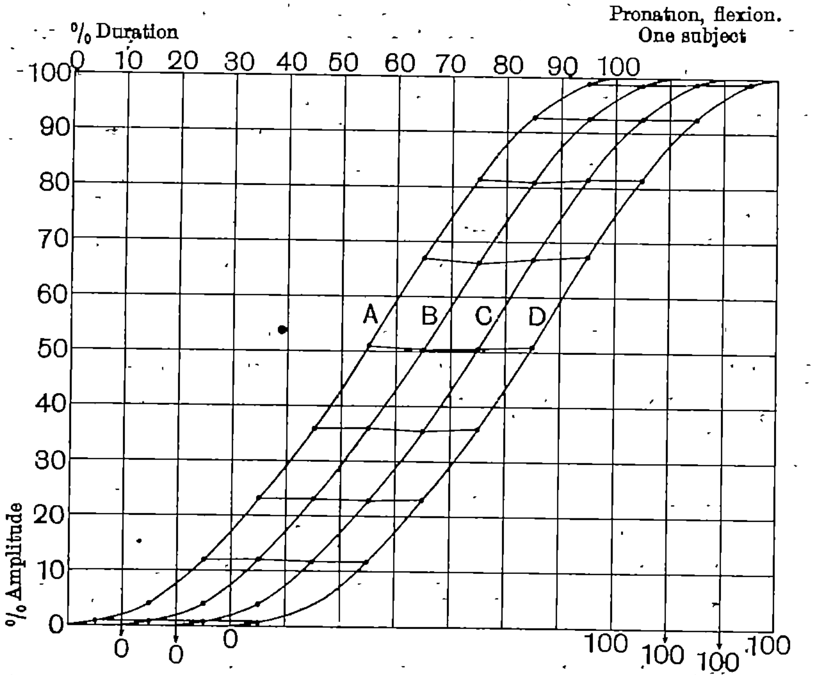
[1] If the curves were identical these horizontal connexions would form straight lines. It is evident there is no uniform deviation of the *B*, *C* or *D* curves from the *A* curve which is the average for the strokes of all lengths. The greatest deviation of the former three respectively from *A* at any point is ·5, ·7 and ·3 per cent. of amplitude, while the mean deviations for the three are ·17, ·21 and ·19 per cent. respectively.



A. 206 unselected strokes (amp. 17–51 mm.) C. 64 medium strokes (amp. 31–37 mm.)
B. 68 short ,, (amp. 17–30 mm.) D. 64 long ,, (amp. 38–51 mm.)

Diagram 6. Progression of short and long strokes with single load.

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[1] In the last analysis, therefore, the length of a stroke seems to be peculiarly determined by the conditions of its initiation and this in turn involves the character of the preceding stroke. For example, from the fact that a stroke may have a progression velocity so unlike (even double or half) that of its predecessor but still exhibits the same characteristic curve of progression, there must be considerable discontinuity in the force applied for contiguous strokes and also there must be some common basis of control which maintains the uniform type of progression.

not only without but also with a load. But it is also the case that they are not entirely pendular, because it was found that the stroke time varied directly as the amplitude with a correlation which was constant and significantly high.

What must be the conditions of muscle innervation and of muscle action which produce these characteristics in reciprocal voluntary movements? In considering this question the facts pertaining to the nature of long and short strokes as here given may be summarised: A subject, exerting his maximum effort to oscillate the wrist through a given excursion as rapidly and accurately as he can, gives a distribution of long and short strokes which occur in an erratic order. The distribution of amplitude as well as the rate of incidence and the form of stroke progression depend upon conditions of position, phase and load; but with these determined, the following points obtain as regards the length of strokes. Introspectively a subject does not exercise deliberate control from one stroke to the next, but maintains the single task of reproducing the standard stimulus. The progression velocity of each stroke, being almost in exact inverse proportion to the amplitude attained, apparently, determines the latter. This rate of progression, which often changes very considerably from stroke to stroke, is imposed in such a way that it is characteristic for the whole stroke, so that the curve of progression (*i.e.* in percentage of amplitude to percentage of duration) is unaltered throughout a series of strokes. Finally this pendular characteristic of wrist action (with or without a load) is qualified by the fact that the duration of strokes varies directly with their amplitude to a significant degree.

One conclusion which seems justified from this is that the amplitude of a stroke is pre-determined once the stroke starts, the pertinent question being how its beginning is affected by the preceding stroke, *i.e.* what are the conditions of phase change as regards reciprocal movement of the wrist?

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THE EFFECT OF BILINGUALISM ON INTELLIGENCE

BY D. J. SAER.

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I. INTRODUCTION.

THE use of the native language in a subject-state tends to weaken in favour of the language of the governing state: yet a people will not readily abandon their language and adopt another. Even where a new language has been generally adopted by a community and the native language has gone out of use, certain forms of the old language will remain as permanent characteristics of the new.

The symbols first learnt for expressive use by the human mind are very persistent, and man is incapable of entirely changing them.

Under British rule there are many peoples who speak other tongues, and, consciously or unconsciously, the English language is coming gradually to prevail in the subject-states of Britain, the natives during the process passing through various stages of bilingualism. In the large towns of England itself there are colonies of people whose mother tongue is alien, and in Wales we have whole counties where the large majority of the people speak Welsh as their mother tongue.

II. THE PROBLEM.

During the last four years I have attempted a somewhat intensive investigation of the effect of bilingualism on the intelligence of people in a subject-state, in districts where the native language is generally spoken, and also in districts where both the native language and that of the governing state are used freely.

The investigation was conducted in seven districts in Carmarthenshire, Cardiganshire and Montgomeryshire. In six of these districts the mother tongue of the majority was Welsh, but the children of the rural districts learn English at the school and those of the urban districts have learnt English both in school and from playmates. The seventh was a rural district with English as the mother tongue. In rural districts a few children whose mother tongue was English had learnt Welsh, but the number of such cases was negligible. All stages of bilingual efficiency were met with. In descending degree of bilingualism the children could be graded thus:

	Mother language	Home language	Play language	School language	Church language
1.	Welsh	Welsh	Welsh	English	Welsh
2.	Welsh	Welsh.	English	English	Welsh
3.	Welsh	English by child's choice after first 3 or 4 years	English	English	Welsh or English
4.	English	English	Welsh	English	English or Welsh
5.	English	English	English	English	English

Five of the districts were rural where the social conditions and habits of the people were very nearly alike. The other two districts were urban and industrial. With very few exceptions the whole child population from 7 to 12 years of age was tested. The headmasters of the schools gave me their ready help. They placed a quiet room at my disposal, and supplied reliable information as to the social position, home language and the age of each child. Nearly 1400 children were examined individually, and as the examinations were all conducted personally, the standard of estimation was uniform throughout.

Every child was tested by means of his mother tongue, as I found, by an experiment on 148 boys who were retested after an interval of more than a year, that even when Welsh speaking boys are able to speak English fluently and prefer it to Welsh, since they use it in their play, their mother tongue is the best oral medium by which we can gain a just estimate of their mental capacity. In rural Welsh speaking districts, when the children were addressed in Welsh, their shyness disappeared, they immediately became interested, natural in attitude and responsive, their best effort being thus readily secured.

In the years 1921 and 1922 I set an Intelligence test to more than 600 older people, students at the University College of Wales, Aberystwyth.

The tests were devised to estimate the relative mental capacity of these persons for purposes of comparison with their academic attainments. On reviewing the results of these tests, after securing information as to the language first spoken by each student and his home address, further evidence was obtained for considering the effect of bilingualism on intelligence and the persistence of that effect.

III. TESTS ON SCHOOL CHILDREN.

The tests on school children are explained under five headings: (*a*) the Stanford Binet scale, (*b*) Dextrality, (*c*) Rhythm, (*d*) Vocabulary, (*e*) Composition. A fuller account of the first four is given in the *Journal of Experimental Pedagogy*, vol. VI, 1922, nos. 4 and 5, except that the figures given in the present article include 53 cases of urban children who were examined subsequently.

(*a*) *The Stanford Binet-Simon scale.*

All the children examined were tested by the Stanford revision of the Binet scale, to which were added those tests in Binet's 1911 scale as well as those in Burt's English version of the Binet which were omitted from the Stanford scale. With the record thus secured, after testing the children individually, it was possible to calculate the mental age of each child according to the three scales. This device proved useful as a check upon errors of calculation in individual cases, and also as a means of observing the consistency of the general tendency of the conclusions.

The test questions were carefully translated into a style of Welsh easily comprehended by Welsh-speaking children, and each child was tested by means of his mother tongue.

In one district children over 11 years 5 months were not tested, but as the Binet scale was found not to give consistent results with children over 12 when they were re-examined after an interval of a year, the record of such children can be ignored for the moment.

The following table of frequencies will represent the difference between monoglot and bilingual children in rural and in urban districts:

Frequencies of I.Q.'s with children 7 *to* 11 *years old.*

Class I.Q.'s	50	60	70	80	90	100	110	120	130	140	Total
Rural monoglots	—	—	8	23	33	39	3	1	—	—	107
Rural bilinguists	9	13	58	110	93	23	4	—	—	—	310
Urban monoglots	—	—	3	21	58	36	20	7	3	2	150
Urban bilinguists	—	—	7	32	92	86	35	4	1	4	261
										Total frequencies	828

The next table gives the median mental age of the same children at each year of physical age 7 to 11 years.

Class	Physical age (yrs.)	7	8	9	10	11
Rural monoglots	Mental age (yrs.)	7	7·8	8·3	9·4	10·2
Rural bilinguists	,, ,,	6·5	7·1	7·8	8·5	9·2
Urban monoglots	,, ,,	7·4	7·9	8·8	9·5	10·5
Urban bilinguists	,, ,,	7 4	8·0	9·0	9·4	10·5

Both these tables indicate the significant inferiority of the bilingual children in rural districts when tested by the Stanford scale, the latter table suggesting that this inferiority becomes consistently greater in degree with each year from 7 to 11 years of age.

The percentile graph for these 828 children gives fairly smooth curves for each of the four classes named, with the following features. The curves for urban monoglots and urban bilinguists are very nearly concurrent, and cross each other five times, while the curve for rural monoglot children is superior to that for rural bilingual children at each percentile. At the median of either curve a horizontal line meets the other curve at a distance of 30 percentiles.

(b) *Dextrality.*

As it was noticed at an early stage of the investigation that bilingual children had not as unerring a sense of dextrality as the monoglots, the difference in this respect was thereafter observed more closely. Note was made of a consistent reversal of sides throughout a test of six responses, and of undue hesitation or apparent confusion before giving the correct response. Correct responses to such a test are generally expected in normal children of 6 years old. It was therefore expected that children of 8 years and upwards would pass this test with ease.

Among 679 bilingual children 8 years old and over I found 70 (*i.e.* 10·3 per cent.) who responded consistently with a reversal of right and left and 103 (or 15·1 per cent.) who were confused or hesitated unduly. Total 173 or 25·4 per cent.

Among 281 monoglot children in the same districts 9 only (*i.e.* 3·2 per cent.) showed reversal, and 15 (*i.e.* 5·3 per cent.) showed undue hesitation. Total 24 or 8·5 per cent.

In the urban districts, considered separately, the percentages for bilingual children were 7·38 per cent. reversals, and 17·6 per cent. hesitation. Total 25 per cent.

With urban monoglot children the reversals were 2·38 per cent., and hesitations 8·33 per cent. Total 10·7 per cent.

These figures show a marked superiority in the monoglots over the bilinguists in their sense of dextrality in both urban and rural districts.

Dr Mott is reported[1] as stating that "Symbolic language is a social heritage in which auditory and visual symbols have been used, especially by civilized races, to express judgments and feelings. The fact that in all right-handed people it is represented in the left hemisphere shows that it has developed with the specialization of use of the right hand."

In every case represented above the bilingual children had begun to learn a second language at an age when they had not acquired an adequate power of expression in their mother tongue, and the linguistic symbols in writing and reading had been presented to them in the second language. Some confusion must have arisen under these conditions, and the greater the affective value of the mother tongue the greater would be the confusion in the rapidly developing mind.

(c) *Rhythm.*

A graduated series of six rhythms was given to 258 unselected boys in an urban school. Each rhythm was tapped on a table with a pencil out of the sight of the boys, who were taken individually in a quiet room. They were then required to repeat the rhythm by tapping it on the table. After the six exercises had been completed, they were again given one by one, but this time the stimulus and the response were by singing the rhythm to 'laa.' Marks with a maximum of 4 for each rhythm were allotted for correctness in 'attack,' length, and consistency of rhythm.

Median Marks obtained in the Rhythm tests.

(Maximum 24 marks.)

	Monoglots					
Age (yrs.)	7	8	9	10	11	12
Tapping ...	8·8	11	14	14·5	17	17·5
Singing to 'laa'	14·8	16	17	18	18	18·5
	Bilinguists					
Tapping ...	10	10	14·6	14·8	14	14·8
Singing to 'laa'	13·4	15·5	16	16·8	16·8	16·8

In the tapping exercise the monoglots were superior at 8, 11 and 12 years of age, but the bilinguists were superior at 7, 9 and 10 years. Yet, when the rhythms were sung to 'laa,' the monoglots were superior at each year of age.

The gain obtained on repeating the rhythms by singing was 3·45 marks per child on the average in the case of the monoglots, while the bilingual boys gained an average of 2·84 marks on the repetition of rhythms by using the organs of speech.

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[1] *Brit. Med. J.*, Dec. 11, 1915.

(*d*) *Vocabulary.*

In order to secure a comparative estimate of the children's range of vocabulary in English and in Welsh, I made use of Anwyl's Welsh dictionary and drew up four lists of 100 words each, selecting every word as it came at the end of even steps taken four times through the dictionary. The 400 words thus found were then arranged in the order of their apparent difficulty to children, and every fourth word in this list was selected to make up the final list of 100 words which was used for testing. The number of words from this list comprehended by a child was taken as indicating the percentage of the Welsh vocabulary that had been acquired by him. The first list of 100 words given in the Stanford scale was used for estimating the child's range of English vocabulary.

It was found that at 10 years of age in rural districts there was a sharp rise in the range of the bilingual children's vocabulary in both English and Welsh, and that the mean ratio of Welsh vocabulary to English vocabulary ranged from 3·47 at 7 years to 2·44 at 12 years of age. In the case of the monoglot English-speaking children in rural districts the most considerable rise in vocabulary appeared at 8 years and the next at 9 years of age.

In the urban districts the sharpest rise in the range of the English vocabulary occurred between 9 and 10 years in the case of both monoglots and bilinguists. The investigation showed (*a*) that the capacity for acquiring a vocabulary increases rapidly between 8 and 11 years of age, (*b*) that the mean range of vocabulary of monoglot children in English was higher than that of bilingual children either in English or Welsh.

(*e*) *Composition.*

In his article on bilingualism and mental development[1] Frank Smith shows that four groups of scholars investigated independently and under observation for two years, developed at a different rate in their power of free composition (in English) according as they were monoglot or bilingual. The superiority in each case, significant in amount, is with the monoglot. A superiority of the monoglots was also shown in the analogies test in three of the four groups. He concludes that monoglot children between the ages of 8 and 11 make better progress than bilingual children in their power of expression, their choice of vocabulary, and their accuracy of thought; and that so far from bilingualism being an "intellectual advantage" it seems to be exactly the reverse, at least under the present organisation of schools in Wales.

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[1] This *Journal*, XIII, 278

To test their power of expression in Welsh and in English I asked children of 9 years and upwards in the various districts examined to write in Welsh a true account of the dream they had last experienced, to write it exactly as it occurred and add nothing that was not in the dream. I impressed upon them that I wanted to find out whether they could give a true and exact account, and that the dreams would be kept private. They were asked to write with a pencil in order to aid their freedom of rapid expression and were told not to bother about the spelling or the neatness of the writing. After a child had completed that exercise, he was asked to write—this time in English—about the thing he liked best of all. He was again allowed to write with the same freedom from spelling and penmanship inhibitions.

Over 400 of these scripts were received. Monoglot English-speaking children, of course, wrote both exercises in English. In many cases the spelling of Welsh was delightful in its independence of the rules of orthography, but very few exercises were found so original in the spelling as to be difficult to decipher. English composition had been taught at the schools but there was no evidence that much serious written composition in Welsh had been attempted. After reading the papers through several times the following features became evident:

(*a*) In the Welsh accounts of the dreams the sentences are generally well-connected and are expressed lucidly in rather good conversational idiom, with completeness of expression and continuity of thought, while the English exercises by the same children are in short sentences disconnected in form and thought, and frequently mixed in the tenses.

(*b*) The choice of subject to represent the thing they liked best indicated a difficulty in thinking outside the groove of thought into which the school lessons in English had led them. Nearly 50 per cent. chose such animals as the cow or horse, stating that it was a four-footed animal with two eyes, one tail, etc.; 45 per cent. wrote about articles of food; the home, the mother, the school, or money in phrases that were bookish and apparently not well comprehended. About 5 per cent. only chose to write about trains, ships, motor bicycles, aeroplanes.

(*c*) Evidence of reasoning in the exercises was very much more frequently met with in the Welsh exercises than in the English exercises of the bilingual children.

(*d*) While the rudiments of an individual style of expression were evident in the Welsh sentences, the English phrases were palpably snatches of school lessons and indifferently memorised verbal expressions that had not as yet been comprehended by the child or adopted by him as his very own, for the disconnected expressions in English can hardly be said to be fully appreciated by him. Not until appreciation has become a significant part of the child's attitude towards any phrase can its adoption by him be looked for with any confidence.

IV. TESTS ON UNIVERSITY STUDENTS.

(*a*) *Tests of* 1921.

At the beginning of the Christmas term 1921, the candidates for entrance scholarships at the University College of Wales, Aberystwyth, the students in the normal department and the ex-soldier special training class worked a 'General Paper' which I drew up to test their intelligence. The normal department consisted of a number of graduates in their fourth year, as well as students in their third, second or first academic year. They formed a group of people who would generally be classed as 'superior adults' who hailed from England and from all parts of Wales.

An effort was made to secure conditions as similar as possible under which the test was worked by each group. The 170 students in the normal department who worked the paper formed a homogeneous group upon whose performance I was enabled to 'weight' the marks, the maximum marks allocated to each question being in proportion to the reciprocals of the logarithms of the number of correct answers given to the question by this homogeneous group.

The validity of the paper as a test of intelligence,—that it tested a mental feature which did not appear to increase by academic training, and would be as evident in a student on his entrance to college as it would be when he completed his course,—is shown by the relative standing of the various groups tested, which were as follows:

1.	32	second-year students in the normal department	120	marks
2.	44	first-year „ „ „	112	„
3.	67	fourth-year „ „ „	110	„
4.	64	candidates for the entrance scholarships ...	103	„
5.	27	third-year students in the normal department	102	„
6.	44	ex-soldier special training students	81	„
Total	278	students		

The class standing was found by calculating the percentage of correct answers made by each group in each of the nine sections of the paper, arranging these percentages in order of value, noting the number of times a group stood first, second, third, etc., and then allocating relative marks for the positions thus secured. The maximum mark, where a group stood first in all sections of the paper, was 126, and the minimum mark 81, where a group stood last in all sections.

On this basis we find that first- and second-year students worked the test as successfully as students in their third and fourth year.

The instructions given, before the test was commenced, included the following: "There are 60 questions, many of them quite easy, and you are not likely to find any question very difficult if you think calmly about it. Sufficient time to complete the paper will be allowed those who work at a fairly rapid rate. Work rapidly and carefully. Do not guess, and do your best to complete the test in the time allowed which will not be over an hour, and it may be less."

The paper was divided into nine sections:

Section 1 consisted of statements which were paraphrases of ten proverbs followed by a list of the proverbs themselves. Students were asked to write after each proverb the number of the statement that they considered best interpreted it.

Section 2 contained ten incomplete analogies which were to be completed by adding an appropriate word to each.

Section 3 contained eight reasoning tests involving simple numbers, *e.g.*

"A man is 15 years older than his brother. In the year 1910 he was twice as old as his brother. How old was the man in 1920."

"A block of wood 4 inches long and 3 inches wide on the top face and 2 inches thick, is painted on its four sides but not on the top and bottom faces. It is then sawn into one inch cubes. (*a*) How many of the cubes will be found painted on two sides? (*b*) How many will be painted on one side? (*c*) How many will not be painted on any side?

Section 4 asked for words having exactly the contrary meaning to 20 given words.

Section 5 gave ten statements, *e.g.*, "We can hardly contend that it is not conducive to fatigue to climb a high mountain." The last word of any statement that was correct was to be crossed out, and the first word of any absurd or false statement had to be underlined.

Section 6 provided exercises in following directions, *e.g.*, "If Scott wrote 'Henry Esmond' cross out the word No, unless you think that Dickens wrote 'Oliver Twist' in which case you should draw a line under that word."

Section 7 asked that the clearest and most pertinent feature in a given well-known book should be described in one word. Four books were then named.

Section 8. In the following questions underline that word in the second group which denotes what is referred to in the first group:

"Upright, crafty, indolent."—"Life, health, character, religion," etc.

Section 9 was a 'Completion test,' twenty-one words being omitted from an extract from a fable; the students were asked to write the appropriate word in the spaces indicated.

Two students completed the paper in 17 minutes. The average time taken was 35 minutes. The median mark obtained was 438 out of a maximum of 655 marks, the standard deviation being 57 marks.

After excluding 44 students in the ex-soldier special training class, which stood last in the list given above, and also 37 students from England, we have 197 students who came up from Wales. Of these 102 had learnt Welsh as their mother tongue and the mother tongue of the 95 other students from Wales was English.

The median mark of the Welsh-speaking students, who, of course, were bilinguists, was 417, while the median mark of the students whose mother tongue was English amounted to 466.

The marks of these two groups at each 10 percentile were as follows:

Percentile ...	Lowest	10	20	30	40	50	60	70	80	90	Highest
Welsh bilinguists	230	317	344	378	396	417	430	452	481	517	578
English monoglots	252	337	393	422	435	466	485	499	511	532	568

A graph constructed from the above figures shows that the standard attained by 50 per cent. of the bilingual students from Wales in this test is attained by 75 per cent. of the monoglots of the same country.

(*b*) *Tests of* 1922.

At the beginning of the present session I drew up another Intelligence test for the candidates taking the Entrance Scholarship examination at the same college, the paper being also taken by students in the normal department as on the previous occasion.

The conditions of the two tests (that of 1921 and 1922) differed as regards the time factor. In the 1921 test, emphasis was laid on speed, while in the 1922 test that was omitted, care being now taken to avoid hurrying.

In the latter paper (*a*) greater prominence was given to the 'Completion' test; two selections were given for completion by writing appropriate words in spaces indicated. One was taken from *On Story Telling* by Richard Steele, and the other—selected by Professor Rose—was from one of Plato's dialogues.

(*b*) The test was more difficult and a longer time was allowed for working it.

(*c*) An attempt was made to estimate the following factors.—Perception of the concrete and verbal perception, ability to control, sustain and concentrate attention; ability to hold in mind the conditions of a problem; ingenuity and practical judgment; steadiness of purpose; ability to use imagery; generalization, reasoning, delayed memory, orientation, and also exactness and rate of mental functioning.

(*d*) An option was given to answer questions either in Welsh or English where there might be a difficulty in clear expression of the response.—Advantage of this option was taken by several students, and difficulties of expression on account of bilingualism were thus to some extent reduced.

In spite of these differences the correlation between the marks obtained by 70 students who took the 1921 test and also that for 1922, worked out by the Bravais-Pearson Product Moment formula was

$$r = \cdot 524 \pm \cdot 09.$$

The 1922 test was worked by 333 students. The shortest time taken by any student to complete was 40 minutes; the longest time taken was 106 minutes.

The mean time taken by the	Upper Quartile	(83 students)	was	72	minutes
„ „ „	Second Quartile	(83 „)	„	83	„
„ „ „	Third Quartile	(83 „)	„	85	„
„ „ „	Lower Quartile	(84 „)	„	84	„

The maximum marks for each question were 'weighted' on the same plan as that for the previous test. The maximum mark for the whole paper amounted to 250. The median mark for all students was 107, and the standard deviation was 17 marks.

By disregarding students coming from places outside Wales, who constituted a 'Selected' class, we have 291 unselected students left for comparison who have come up from all parts of Wales. This provides a sufficient number of cases for dividing up into four groups similar in nature to the groups under which the children were considered in Section III.

82 students were bilinguists from rural districts in Wales. Their mother tongue was Welsh.

45 were monoglots with English as their mother tongue. These students again came from rural districts in Wales.

72 were bilinguists having Welsh as their mother tongue from urban and industrial districts in Wales.

92 were monoglot English-speaking students from urban and industrial districts in Wales.

The following table of frequencies shows how these four groups stand. The frequencies are worked out in steps of 17 marks (the standard deviation) counting on either side from the median mark—107.

Marks ...	39	56	73	90	107	124	141	158	175	No. of cases
Rural Welsh bilinguists	2	6	16	22	21	14	1	0	0	82
Rural English monoglots	0	1	9	9	12	12	2	0	0	45
Urban Welsh bilinguists	0	2	12	14	21	15	4	4	0	72
Urban English monoglots	0	2	12	21	25	19	8	4	1	92
	2	11	49	66	79	60	15	8	1	291

The curves representing these numbers in a graph show a clear superiority of the monoglot over the bilingual student coming from rural districts in Wales, while the difference between these linguistic groups from urban and industrial districts is inconsiderable.

The marks (maximum 250) at each percentile representing the same groups are given in the following table:

Percentile ...	Lowest	10	20	30	40	50	60	70	80	90	Highest
Rural Welsh bilinguists	52	73	85	92	100	105	111	117	123	131	147
Rural English monoglots	71	78	85	92	97	113	120	127	138	140	147
Urban Welsh bilinguists	68	80	90	96	107	111	117	128	134	148	164
Urban English monoglots	67	86	98	104	107	114	119	128	137	146	170

The total number of English-speaking monoglot students from the rural districts of Wales being small, 45 only, the frequency curve as well as the percentile curve representing the performance of this group is somewhat irregular, but the superiority of the curves of performance of this group over those of the Welsh bilingual students from similar districts is clearly seen, and it becomes still more emphatic when the curves are 'smoothed.'

It will be readily granted that the language a child uses in his play and in his associations with youthful companions is charged with a high degree of affective value, and that this language occupies his consciousness by preference throughout the period of childhood. Where the language of play is also that of the home and of the church, as is the case with Welsh-speaking children in the rural areas of Wales, the child's word symbols are formed in that language with a strong affective tone.

In the urban and industrial districts of Wales, bilingual children very generally adopt English as the language of play. Where Welsh is the language of the home and the church in such districts, it is found that the language used in play tends to invade the home and the church, and gradually to become that in which a child responds to his parents even when they address him in Welsh. The children also use English in their conversation with one another in the home in preference to Welsh. Under such circumstances any emotional conflict between the use of Welsh and English that may arise is resolved by the child at an early age, and English is used by him on every available occasion. In urban and industrial districts the child who first learnt Welsh at his home and subsequently learnt English from his play companions thus enjoys almost the same emotional freedom from language conflict as the monoglot in the same district, for play has some cathartic influence in its tendency to relieve the tension due to the dominance of the '*reality principle*.' The child can thus enter upon his school duties and find little difficulty in adopting as his own the English word symbols there used. The English vocabulary of these children, however, does not carry as fully symbolic a significance as that of the English monoglot, as there has been some confusion and lack of definiteness in the meaning through the substitution of a word in the second language for that of the mother tongue, where the word was more fully charged with affect. The words 'home' and 'bread' cannot be expected to have the same fulness of affective tone to a Welsh-speaking child as the words 'cartref' and 'bara' that he first called these objects by. This possibly accounts for the difference found by Frank Smith in his investigation referred to in Section III *e*, and in the tests for dextrality, rhythm and composition made upon urban children and described in Section III *b*, *c* and *e* of this article.

The Welsh-speaking child in rural districts begins to use English symbols at school in reading and writing before he has learnt to express himself adequately in his mother tongue. Welsh is somewhat neglected or almost entirely ignored and English is learnt slowly and laboriously. Since the Welsh symbols that are ignored have for him a high affective tone, and since the cathartic influence of play does not operate, for he uses Welsh in his play, a conflict must arise between his self-regarding sentiment or positive self-feeling and his negative self-feeling or his instinct for submission. I suggest that this may account for the large difference found between bilinguists and monoglots in rural districts in this investigation as compared with the smaller difference between bilinguists and monoglots in urban and industrial districts.

V. CONCLUSIONS.

1. Monoglot children in rural districts in Wales show a considerable superiority over bilingual children in the same districts when tested by the Binet scale of intelligence.

2. University students coming from rural areas in Wales show the same differentiation in a test of intelligence.

3. This difference appears to be of a permanent nature since it was seen to persist in students throughout their university careers.

4. In rural districts the sharpest rise in the curve for vocabulary tends to occur with monoglot children at 8 or 9 years of age. In the case of bilingual children this is postponed until 10 or 11 years of age.

5. The range of vocabulary of monoglot children is higher than that of bilinguists (either in Welsh or in English).

6. When writing English the Welsh bilingual child uses his memory to recall alternate word symbols in two languages instead of his reasoning power which should make the image clearer and more significant to him. He thus exercises himself with indefinite symbols during a period when his disposition is to be precise and definite.

7. The results of the tests for dextrality suggest that a confusion has been carried over from the brain area concerned with language to the related specialised area connected with the use of the right hand in gesture or in writing. The bilinguists were found to be much more confused in this respect than monoglots both in rural and in urban districts.

8. Children in urban districts tested by the Binet scale and university students from similar districts in Wales estimated for intelligence by a group test show an inconsiderable difference between monoglots and bilinguists.

9. But mental confusion is seen to exist in bilingual children to a higher degree than in monoglot children even in urban areas by the tests for dextrality, and also in the rhythm tests when the organs of speech expression functioned in singing a series of rhythms to 'laa,' the monoglots showing a consistent superiority over the bilinguists at each year of age from 7 to 12; a difference that did not appear in the same rhythms when the children responded by tapping.

10. It is suggested that the greatest influence on the mental confusion occurring in bilingualism is exerted by the language used by bilingual children in their play and in their free association with youthful companions when that language is not also that in which they are first taught at school.

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SOME MEMORY EXPERIMENTS WITH HIGH-GRADE DEFECTIVES[1]

BY LUCY G. FILDES.

Experiment 1. *Learning forms and associated names.*
Procedure (pp. 40–41).
Results (pp. 41–48).
Experiment 2. *Learning forms only.*
Procedure (p. 49).
Results (pp. 49–51).
Experiment 3. *Learning names only.*
Procedure (pp. 51–52).
Results (pp. 52–54).
Experiment 4. *Learning sense material.*
Procedure and Results (pp. 54–55).
Summary and General Conclusions (pp. 55–56).

THE experiments about to be described record an attempt to discover whether any characteristic differences exist between the learning power of a defective and that of a normal child. More especially they aim at investigating the nature of such differences with the type of learning most commonly in primary school work.

The question is an important one, especially in view of the increasing seriousness of the problem of the education of the high-grade defective, a problem impossible of solution in our present absence of exact knowledge as to the psychological nature of mental defect.

The subjects for this experimental work were:

(*a*) fifty defective boys resident at Littleton House School, Cambridge;

(*b*) various groups of normal children selected for purposes of comparison.

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[1] Read at a General Meeting of the British Psychological Society, 28 January 1922.

EXPERIMENT 1.

Procedure.

Eight simple forms (see Table V, p. 45) were chosen and associated arbitrarily with eight nonsense words as names.

The following names were used in this experiment:

(1) ek. (2) nop. (3) dak. (4) lut. (5) itta. (6) ena. (7) na. (8) sor.

Each form was either (*a*) drawn on a card (forms 1, 2, 5, 6) or (*b*) cut on a piece of wood (forms 3, 4, 7, 8) four inches square.

The subjects were required to learn the name of each form in the same way as a child is required to learn the name of a letter, *i.e.* so as to be able (*a*) to give the appropriate name when the form is shown and (*b*) to draw the appropriate form when the name is given.

They were taught individually—the method of procedure in teaching being exactly similar in each case. This briefly was as follows:

1. A form was shown to the subject for a period of 5 secs. At the same time its name was said by the experimenter and repeated by the subject. This was done three times. Each form was dealt with in this way until all had been so exposed and named.

2. Either (*a*) a name or (*b*) a form was presented to the subject and he was required to draw the appropriate form or to give the appropriate name. This was repeated until all the eight forms and the eight names had been presented.

3. The procedure in (1) and (2) was repeated until the subject could give correctly all the forms and all the names during one attempt at reproduction, or, in the case of continued failure, until twenty-four attempts at reproduction had been made.

4. Three presentations of the forms and two attempts at reproduction were made at the first sitting. At all subsequent sittings two presentations and two reproductions were made, so that each such sitting ended with a correct presentation of the forms and began with an attempted reproduction.

5. Sittings were given to each subject as far as possible at intervals of one day throughout the learning process.

6. The order of the presentation of the forms in learning and of the names and forms in reproduction was changed on each repetition of the learning process. The same order, was, however, maintained with all the subjects for each successive repetition.

7. The method of presenting the different forms varied. At two forms—ek and nop—the subject only looked while naming them: two—dak and lut—he traced blindfold: two—itta and ena—he drew: and two—na and sor—he traced while looking at them. The aim of this procedure was to see whether the different method of presenting the forms affected the ease with which they were reproduced.

8. Any drawing made by a subject in reproduction was removed immediately from his sight.

The subjects for this experiment were:

(*a*) 30 mentally defective boys of varying ages—from 7 yrs. 2 mths. to 15 yrs. 11 mths. (Average age = 12 yrs. 3 mths.)

(*b*) 25 normal boys between the ages of ten and twelve years. (Average age = 11 yrs. 1 mth.)

Results.

A. *The number of repetitions required for learning.* The average numbers of repetitions required for successful learning by the boys in the two groups are shown in Table I. It must be remembered that one repetition includes three presentations of each form.

TABLE I.

Showing the number of repetitions required for learning.

	30 M.D. boys		25 M.D. boys		25 Normals	
	Yrs.	Mths.	Yrs.	Mths.	Yrs.	Mths.
Aver. age...	12	3	13	0	11	1
Aver. no. of reps. required for learning	13·3		11·4		5·64	

Note. If the results of the five defective boys under the age of ten years are not included in the results (*i.e.* if the youngest of the defectives is at least as old as the youngest of the group of 25 normals—to eliminate any possible age difficulty), the average age of the group of defectives is raised to 13 yrs. and their average number of repetitions is lowered to 11·4 as is shown in the above table.

Individual variations in the rate of learning shown within the groups were:

30 M.D. boys	5–24 repetitions
25 „	5–24 „
25 Normals	2–9 „

From the results given above it is possible to draw certain conclusions with regard to the comparative learning ability of normals and defectives under the conditions of the experiment:

1. The defectives are on the whole slower than the normals, for they require on the average twice as many repetitions for successful learning.
2. They show a far greater individual variation in ability than is shown by a parallel group of normal children.
3. The power to learn among defectives shows a fairly high correlation with mental age. (r = ·63, p.e. = ·077.)

B. *The relation between success in learning names and forms.* Under the conditions of the experiment all the names had to be correctly given and all the forms correctly drawn in one reproduction before the work was considered to be known. It is, however, easily conceivable that the power to name and the power to draw might not have developed equally rapidly in all cases. Yet the results show but little difference in the rate of development of the two powers. One boy—a defective—could draw the eight forms before he could name two of them: six defectives could name the eight forms before they could draw them all. Two normal boys could draw the eight forms before they could name them all.

Table II shows the relative development of these two powers more exactly.

Table II.

Showing the percentage of the possible number of correct answers given on each successive reproduction.

Reps.	1	2	3	4	5	6	7	8	9	10	11	12
Naming:												
Defects.	0	12	19	28	33	45	42	55	46	59	54	65
Normals	15	32	40	67	52	86	87	94	100	—	—	—
Drawing:												
Defects.	0	15	15	27	22	31	33	43	39	44	44	56
Normals	38	42	62	52	83	80	91	100	—	—	—	—

The defectives here resemble the normal boys in that ability to name and to draw develops at about the same rate.

C. *Relation between success in reproduction and the method employed in learning the forms.* As has been indicated, four different methods of presenting the forms were used, viz.: (1) Visual; (2) Tracing blindfold, (3) Drawing, (4) Tracing seeing—with the aim of discovering whether the method of presentation affected the rate of learning. Table III gives the results from this point of view.

TABLE III.

Showing the percentage of possible scores made for each form.

Mode of presentation	Visual		Motor (Blindfold)		Drawing		Motor (Seeing)	
Name of Form	Ek	Nop	Dak	Lut	Itta	Ena	Na	Sor
Normals:								
Naming	28	45	50	57	35	58	42	71
Drawing	35	44	53	52	46	55	44	71
Defectives:								
Naming	48	68	39	41	38	54	35	80
Drawing	36	54	19	23	43	52	34	68

Or, to express the same facts in a different way, the following list shows the relative ease with which the different syllables were learnt.

Names of forms in order of ease of learning.

	Normals		Defectives	
	Naming	Drawing	Naming	Drawing
1.	sor	sor	sor	sor
2.	ena	ena	nop	nop
3.	lut	dak	ena	ena
4.	dak	lut	ek	itta
5.	nop	itta	lut	ek
6.	na	na	dak	na
7.	itta	nop	itta	lut
8.	ek	ek	na	dak

No indication is given in these results that the method of learning in any way affected the ability to learn, except for the fact that in the case of the defectives the forms learnt blindfold are reproduced with the greatest difficulty. Observation and also the results of some later work suggest that this is due, not so much to the method of learning, as to the fact that the method of learning was a new one. The defective boys found it hard to realise what was wanted. They did not attend to their movements. The normal boys, on the other hand, attended most keenly here, because the method was new.

If, therefore, the method of presentation does not influence success, some other factor must be operative. A suggestion as to the nature of that factor is offered by the fact that the form associated with sor is most readily learnt by practically all the boys. Here the nonsense syllable was also a real word, and so provided a ready basis of association in itself. One of the defective boys well expressed the reason for its ease "I ought to know that, the wood is sawn."

We may conclude, then, that:

1. Both normals and defectives depend rather on the nature of the syllable (and of the form) for successful learning than on the method of presentation, except when the method of presentation is new.

2. In that case the defective finds the mode of presentation a hindrance to learning because of his failure to adapt himself to the new elements in the method.

D. *Errors.* Successful learning is marked, not only by ease of acquirement, but also by the ability to avoid errors in reproduction. We have seen that the learning of high grade defective boys is poor in that they require, on the whole, far more repetitions for successful learning than do their normal fellows. How does their power stand with regard to errors other than errors of omission?

Positive errors possible in this experiment are of two types:

1. A name or form may be given which really belongs to the experiment but which is wrongly associated, *e.g.* may be called dak or itta drawn .

2. A name or form not belonging to the experiment may be given, though in this case:

(*a*) it may resemble one of those belonging to the experiment and be obviously derived from it: *e.g.* mop for nop, luk for lut;

(*b*) it may lie altogether outside the names and forms used in the experiment.

Tables IV and V show the number and types of errors for the two groups.

TABLE IV.

Showing errors in drawing.

DEFECTIVE BOYS (29).

Drawing given in response to	ek	nop	dak	lut	itta	ena	na	sor	Not in exp.	Total
ek	—	3	3	1	64	36	15	7	11	140
nop	30	—	2	2	14	26	14	5	4	97
dak	22	3	—	16	42	51	17	16	18	185
lut	15	1	21	—	25	39	14	8	19	142
itta	25	—	4	3	—	59	19	8	5	123
ena	16	3	—	3	46	—	9	14	10	100
na	16	14	—	7	34	47	—	13	13	144
sor	1	—	2	—	18	10	30	—	6	67
Total ...	125	24	32	32	243	268	118	71	86	

NORMAL BOYS (25).

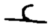
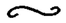
Drawing given in response to	ek	nop	dak	lut	itta	ena	na	sor	Not in exp.	Total
ek	—	6	3	7	9	11	4	2	4	46
nop	10	—	3	1	4	10	6	1	—	35
dak	6	4	—	9	4	8	10	2	—	43
lut	3	1	21	—	3	4	1	3	—	36
itta	8	8	1	1	—	16	5	5	1	44
ena	8	5	3	4	11	—	4	3	1	38
na	14	5	5	2	8	8	—	7	—	49
sor	1	1	—	1	—	4	10	—	—	17
Total ...	50	30	36	25	39	61	40	23	6	

Faulty orientation is not counted as an error in drawing.

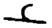
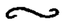
TABLE V.

Showing errors in naming.

DEFECTIVE BOYS (29).

Name given in response to	ek	nop	dak	lut	itta	ena	na	sor	Not in exp.	Total
	—	37	22	4	12	17	9	6	34	141
	7	—	5	3	7	8	8	3	53	94
	24	9	—	34	4	18	5	5	24	123
	14	7	17	—	3	22	5	3	26	97
	21	9	7	14	—	53	16	11	41	172
	17	8	4	10	42	—	1	11	35	128
	17	8	20	12	10	25	—	42	44	178
	5	4	4	3	5	13	6	—	14	54
Total ...	105	82	79	80	83	156	50	81	271	

NORMAL BOYS (25).

Name given in response to	ek	nop	dak	lut	itta	ena	na	sor	Not in exp.	Total
	—	10	7	2	16	7	5	3	13	63
	6	—	3	1	2	8	9	2	11	42
	1	3	—	6	1	3	—	1	6	21
	—	6	—	—	1	3	—	—	6	16
	8	5	1	3	—	20	10	1	17	65
	3	4	1	5	10	—	1	7	2	33
	2	5	8	1	6	7	—	15	16	60
	4	8	2	2	2	8	3	—	2	31
Total ...	24	41	22	20	38	56	28	29	73	

By percentaging the results expressed in these tables in order to get a more exact comparison between the two groups of boys, *i.e.* by estimating the number of errors 100 boys of each kind would make if the ratio of error here shown were maintained, we find the following:

TABLE VI.

Showing estimated number of errors for (a) a group of 100 *defectives and (b) a group of* 100 *normals.*

	ek	nop	dak	lut	itta	ena	na	sor	Total
Defectives:									
Drawing	483	334	638	490	424	345	496	231	3441
Naming	486	324	434	334	593	441	614	186	3402
Normals:									
Drawing	184	140	172	144	176	152	196	68	1232
Naming	252	168	84	64	260	132	240	124	1324

The group of defectives, therefore, shows a tendency to make positive errors which is almost three times as great as that of a group of normals of a much younger average age.

The two main types of errors have already been indicated. It will be observed, however, that errors of type 2 (names or forms given which do not actually occur in the experiment) appear much commoner in the process of naming than in that of drawing. This is probably rather an apparent than a true difference. It is impossible with drawings of this kind to distinguish between forms exactly like the copies and those resembling them in most particulars. In fact, practically no reproduced forms are *exactly* like the copies. With a name, however, the matter is different: it is easy to tell whether the reproduction is exact or only similar, and since only exact reproductions are here counted as right, the number of unclassified errors is largely increased because of this. Indeed, of the 73 errors in naming made by the normal boys, only seven cannot be traced to one of the given names; while of the 271 wrong names given by the defectives, all but fifty-two are directly traceable to the same source. These fifty-two names include twenty-six real words.

The errors which can be traced to names actually occurring in the experiment are made by:

1. Changing the initial consonant of words; *e.g.* lak, vak, sop, sa.
2. Changing the final consonant; *e.g.* nob, luk, dag.
3. Changing the vowel; *e.g.* dik, uk, ik.
4. Omitting the initial consonant; *e.g.* ak, ah.
5. Prefixing a consonant; *e.g.* sek, sena, vena.
6. Altering double consonants; *e.g.* ekka, egga, igga, etc.

All types of changes occur with both groups of boys but all are commoner with the defectives.

An examination of Tables IV and V shows clearly that certain names and forms occurring in the experiment are far more commonly used in error than are others. Such a fact must have some explanation, which can only be that certain forms or names are retained more readily than others, either because of some peculiarity in themselves or because of the way in which they were learnt, and that they are remembered in isolation and not as associated with another specific name or form. Thus being remembered as belonging to the experiment and yet not as belonging particularly to anything special in it, they tend to come up whenever memory fails, and—in the absence of the knowledge that they are wrong—to be given in place of what is forgotten.

Table VII shows the number of times each syllable or form is wrongly used in this way.

TABLE VII.

Forms wrongly given		Names wrongly given		
Defectives	Normals	Defectives	Normals	
125	50	110	28	ek
24	30	94	45	nop
32	36	138	30	dak
32	25	91	27	lut
233	39	104	48	itta
263	61	171	57	ena
118	40	68	33	na
71	26	87	29	sor

The points which determine such isolated retention appear to be:

1. Learning by drawing, especially with the defectives. Possibly this is because the drawing of a form compels an analysis of it which is gained in no other way. The effort to make this analysis seemed, however, often to prevent the association of the form and its name. It is retained, but it is retained as a form 'learnt by drawing' and not as having a special name. In the same way the name is associated with the fact of drawing rather than with the form itself.

2. Some marked definiteness in shape, or some distinctive letter in a name, *e.g.* k in dak and ek.

The results show that the defective boys are much more subject to this type of error than are the normals. Unable to give a correct answer, they reply with anything that comes to mind, *i.e.* in this case with a form or name which has been retained fairly easily. Sometimes, even, the same form (always a form learnt by drawing) is drawn repeatedly for all the names—a type of error never found with normals.

Another possible source of error lies in the chance that certain names or forms may tend to become actually confused with each other because of some intrinsic resemblance. An analysis of the number of times each name or form is given for every other name or form should suggest how far this factor is operative here. Such an analysis is given in the following Table VIII.

The numbers marked * appear certainly to indicate some real confusion between the names and forms concerned. In view of the very frequent use of itta, ena, ek and dak by defectives, and of ena and ek by normals, it is, however, difficult to make any further definite statements on this point. It is interesting that here the basis of similarity and therefore of confusion lies in four cases (marked †) in the method of learning and not in any inherent similarity in the forms or names themselves. An association appears to be made between a name or form and a method of learning rather than between a name and a form. Only in one case has an actual resemblance in form led clearly to confusion.

TABLE VIII.

Showing the possibility of errors being due to confusion of names or of forms on a basis of some definite similarity.

Note. 1. Both names and forms are considered together.
2. The numbers in the normal group are altered so as to make possible an accurate comparison with those of the defective group.

Names or Forms given for each other	Number of times such substitution was made	
	Defectives	Normals
†itta and ena*	215	69
itta „ ek*	158	60
†dak „ lut*	102	46
†na „ sor*	98	43
†ek „ nop*	80	38
ena „ ek	87	36
itta „ na	80	36
nop „ na	58	35
nop „ ena	52	32
ek „ na	60	31
dak „ na	55	31
nop „ itta	32	27
ena „ sor	51	25
ena „ na	81	24
dak and ek	80	23
lut „ ena	76	21
dak „ ena	80	18
ek „ lut	37	17
nop „ dak	24	17
nop* „ sor	19	14
nop „ lut	16	12
lut „ itta	48	12
ek „ sor	20	12
itta „ sor	42	12
dak „ itta	73	10
lut „ sor	14	8
lut „ na	40	7
dak „ sor	27	6

A third possible source of error is not shown in the results, but was obvious in conducting the experimental work. Certain of the boys showed at first a constant tendency to reply to a presented form with the name last given by the experimenter. This tendency was not found among the normals, but was a serious cause of error among the defectives.

We may conclude that

1. Positive errors are mainly of two types:

(*a*) those due to confusion between similar names and forms existing in the experiment;

(*b*) those due to confusion between forms or names of forms learnt in the same way;

(*c*) substitution of some entirely other form for the one required.

2. The errors made by normals and by defectives are of the same kinds, but

3. Defectives make far more errors than do normals.

4. Defectives are apt to be controlled by automatism in reproduction.

EXPERIMENT 2.

The special aim of both experiments 2 and 3 was to discover (*a*) how far, if at all, an associative factor added to the difficulty of learning, and (*b*) in particular, whether the introduction of this factor affected defectives more or less or in about the same degree as it affected normals. To this end a series of forms only (experiment 2) and a series of names only (experiment 3) were taught.

Procedure.

The forms in experiment 2 were similar in nature to those in experiment 1 and were taught in exactly the same manner; that is

1. Two were taught visually, two by tracing blindfold, two by drawing and two by tracing with open eyes.
2. The order of exposure was irregular on each repetition of the learning process.
3. The forms were each shown or drawn three times on each presentation, the period of exposure being 5 secs.
4. The number of repetitions at each sitting, and the length of interval between the sittings was the same throughout as in experiment 1.

The forms used were similar to those of the preceding experiment:

ral	et	Presented visually
mot	asa	Traced blindfold
ik	tup	Drawn
rit	issi	Traced seeing

Note. The names are given merely for convenience of reference. They were not mentioned to the subjects.

When required to attempt a reproduction, the subjects were told to draw any of the forms that they could remember.

The subjects of this experiment were:

(*a*) 28 of the 30 defective boys used in experiment 1.

Average age = 12 yrs. 6 mths. (the experiment was carried out three months later than experiment 1).

(*b*) 25 normal boys between the ages of ten and twelve years.

Average age = 11 yrs. 2 mths.

Results.

A. *The number of repetitions required for learning.* These are shown in the accompanying table.

TABLE IX.

Showing the number of repetitions required for learning.

Experiment 2		Experiment 1	
Defectives	Normals	Defectives	Normals
6	2·3	13·3	5·64 = aver. number of reps. required

The ratio between the average number of repetitions required by the defectives and by the normals is practically the same in the two experiments, but the average number required in experiment 1 is more than twice that required in experiment 2. This difference must be attributable to the additional factor in experiment 1, *i.e.* the name.

B. *The relation between success in reproduction and method of learning.* This is shown, as in experiment 1, by estimating the percentage of successful scores gained for each form.

TABLE X.

Shewing the Relation between Success in Reproduction and Method of Learning. The numbers indicate the number of scores gained expressed as a percentage of the possible scores.

Form	Defectives	Normals	Method of Learning
ral	52	71	Visual
et	67	79	„
mot	54	86	Tracing blindfold
asa	34	79	„ „
ik	63	79	Drawing
tup	61	86	„
rit	40	57	Tracing seeing
issi	53	78	„ „

Here, as in experiment 1, no clear relationship is shown between success in reproduction and method of learning, although here too, the defectives fail most in one of the forms learnt blindfold while the normals are very successful with both of these. Factors other than the way in which the forms are learnt must therefore be the chief determinants in successful reproduction. The place occupied by the form rit suggests one of the most important of these. Rit was the most complicated form used: failure was chiefly due to the lack of appreciation of the relationship of the lines to each other, even when the number of lines were known. Further, it had no clear meaning, a factor which materially helped in the reproduction of ral "like a question mark," and of issi "the way to go—a scout sign," and also serves to explain the comparative success of the defectives with the form mot, learnt blindfold. It was "like W."

An attempt was made to see whether the method which the boys employed to aid reproduction as described by themselves from their own introspective analysis threw any light on the cause of success, but it gave no satisfactory result. The normal boys appeared to succeed however they learnt and all used different methods for different forms: the defectives failed to give any useful analysis of their mental processes.

The conclusions which can be drawn here support those drawn from experiment 1. In addition it can be inferred that:

1. The association of a name with a form increases the difficulty of reproducing it about equally for normals and for high-grade defectives.

2. Learning is helped chiefly by the ease with which meaning can be given to the form to be learnt.

3. The more complicated the form, the more difficult is it to remember.

EXPERIMENT 3.

The aim of this experiment has already been stated (see experiment 2).

Procedure.

Here, instead of eight forms being taught alone, eight names were taught alone (*i.e.* eight nonsense syllables) in the following manner:

1. The experimenter said one syllable and this was repeated by the subject. Then the second syllable was named and repeated, and so on to the eighth syllable.

2. The subject was asked to give any syllables remembered.

3. The procedure in 1 and 2 was repeated until all eight syllables were given by the subject in the order of learning.

4. Ten repetitions were made in one sitting. Sittings were at twenty-four hour intervals as far as could be arranged.

It must be remembered that, while in the previous experiments one repetition or learning included three presentations of each form, in this experiment each repetition involved only a single presentation.

The results are not, therefore, strictly comparable with those of the earlier experiments in certain respects.

The method of teaching the syllables orally was used because most of the defectives found reading a great difficulty.

These subjects fall into three groups:

(*a*) Twenty-four defective boys, including twenty-one who had done the two earlier experiments.

Average age = 11 yrs. 11 mths.

(Youngest 7 yrs. 9 mths. Oldest 15 yrs. 3 mths. All but five of the children over ten.)

(b) Thirty-seven normal girls.
Average age = 10 yrs. 10 mths.
(Youngest 8 yrs. 2 mths. Oldest 13 yrs.)

(c) Forty-three normal boys.
Average age = 8 yrs. 11 mths.
(Youngest 8 yrs. 1 mth. Oldest 10 yrs. 6 mths.)

The following syllables were used:

mip, ret, san, boc, duf, gik, jal, neg.

Results.

A. *The number of repetitions required for learning.* Note. Three repetitions here equal approximately one repetition in the earlier experiments.

TABLE XI.

Showing the average number of repetitions required for learning by the different groups.

	Average age Yrs.	Average age Mths.	No. of repetitions	Range of variations
Defectives	11	11	29	10–43
Normal girls	10	10	8·4	3–18
" boys	8	11	16	4–34
" girls and boys together	9	10	12·2	3–34

Taking the normal children in groups according to age, the average number of repetitions required were:

Years	Average age Yrs.	Average age Mths.	Repetitions
8–9	8	8	16
9–10	9	4	13
10–11	10	2	10
11–12	11	6	7·8

while those for the defectives of corresponding ages were:

8–9	8	1	26
9–10	9	3	34
10–11	10	1	27
11–12	11	4	25

Owing to the different conditions of this experiment exact comparison with the two earlier pieces of work is not possible, but certain conclusions in relation to them can be drawn:

1. The absence of the associative factor in learning has reduced the number of repetitions required for success. For the normal children the average number of repetitions may be considered equal to those required for experiment 2, *i.e.*

Experiment 2. Average age = 11 yrs. 2 mths. Average number of Reps. = 2·3.

Experiment 3. Average age = 11 yrs. 2 mths. Average number of Reps. $= \frac{7\cdot8}{3} = 2\cdot6$.

The defectives, however, are slower in learning the names than in learning the shapes.

2. While a certain relation exists among the normals between rate of learning and age (see above), no such relation exists among the defectives. In fact, the correlation between the ability of this group of defectives in learning nonsense syllables and their chronological-age is a negative one.

(r. = − ·29, p.e. = ·13.)

Further, no correlation can be found between success here and mental age.

(r. = ·098, p.e. = ·14.)

This is interesting in view of the high correlations found in experiment 1.

B. *Other factors in learning.* Further comparison between the work of defectives and that of normals was made under the following headings:

1. The average number of right answers given per repetition by each individual.
2. The average number of wrong answers given.
3. The total number of answers given.
4. The number of actual words given in error.
5. The number of repeated answers given on an average in each attempted reproduction.

The results of these comparisons are shown in Table XII.

TABLE XII.

-Showing the average number of answers of different kinds given per repetition by each individual.

	Right answers	Wrong answers	Total answers	Actual words	Repeated syllables
Defectives	3·3	2·07	5·78	1·206	·39
Normals together	4·6	1·01	5·8	·35	·25
Normal girls	4·8	1·1	6	·38	·18
„ boys	4·4	·92	5·6	·33	·33

The defectives give on an average as many syllables as do the normals on each reproduction, but, comparing their results with those of the younger groups of normals, they

(*a*) give fewer right and more wrong answers;
(*b*) give more actual words;
(*c*) tend to repeat words and syllables given more frequently.

Note. The actual number of words given by defectives = 1306, including repetitions of the same word on different reproductions.

Estimated proportionately (*i.e.* for 24 children in each case) the normal boys give 365 words, and the normal girls 239.

C. *The relation between the position of the syllable in the series and success in reproduction.* Table XIII shows the percentage of possible right answers given for each syllable.

TABLE XIII.

	mip	ret	san	boc	duf	gik	jal	neg
Defectives	45	26	50	49	34	21	50	51
Girls	69	48	80	61	59	36	59	64
Normal boys	53	47	67	48	49	28	67	61
Normals together	61	48	73	55	54	32	63	63

We conclude that

1. The positions suggested as most favourable for reproduction under the conditions of the experiment are the end ones. Almost all the boys tended to begin their first reproductions with the syllable last heard, and many attempted to give the series backwards.

2. Apart from position, the nature of the syllable appears most important (cp. experiment 1). A syllable readily suggesting a meaning, *e.g.* san (sand) is easy: one suggesting several meanings, *e.g.* duf (muff, buff, cuff) is more difficult since confusion of ideas results: one not easily suggesting any word with meaning at all, *e.g.* gik, ret, is harder still, especially if, like gik, it is hard to say. In this case attention and effort go to controlling speech and not to learning the word.

EXPERIMENT 4.

For purposes of comparison with experiment 3 especially, in order to test the effect of meaning as an aid to learning with both defectives and normals, a rhyme—four lines of 'The Owl and the Pussy Cat'—was taught to two of the groups of children who had learnt the nonsense syllables, *i.e.* to

(*a*) 23 defective boys—(1 of the earlier group omitted), and
(*b*) 37 normal girls.

Procedure and Results.

The method of teaching was again individual and oral and exactly similar in all cases. The results are given in Table XIV.

TABLE XIV.

	Average number of repetitions	Range of variations	Average age Yrs.	Average age Mths.
Defectives	6	2–14	11	6
Normal girls	2·1	1–4	10	10

Calculating from these results, we find that:

1. In learning the rhyme the success of the defective boys is in about the same ratio to that of the normal girls as it is in learning the nonsense syllables (about 1 : 3).

2. The defectives show a correlation of ·53 (p.e. ·10) between the number of repetitions required for learning the nonsense syllables and those required for learning the rhyme.

(*Note.* The variability of the normals is not sufficiently great to permit of the classification necessary for working a correlation.)

3. The correlation for the defectives between mental age and ability in learning the rhyme = ·76 (p.e. = ·03), between actual age and the same ability = ·61 (p.e. = ·05).

4. The correlation for the defectives between ability in learning the rhyme and success in experiment 1 = ·47 (p.e. = ·13), and between ability in learning nonsense syllables and success in experiment 1 = ·20 (p.e. = ·06).

SUMMARY AND GENERAL CONCLUSIONS.

1. The groups of high-grade defective boys dealt with are always slower on the average in learning than are corresponding groups of normal children of a lower average age.

2. The defectives of a certain age are always more variable in ability than are normals of that age in all the types of learning employed.

3. In most types of learning a fairly high correlation between ability and mental age is suggested.

4. Defectives give more wrong answers than normals, though their errors are mainly like those of normals in type.

5. The learning of defectives very closely resembles that of normals in many ways, *e.g.*

(*a*) The ability to draw and to name develops at the same rate in both (experiment 1).

(*b*) The method of learning does not appreciably affect the results in either case, except that the defectives fail with a new method through inability to adapt themselves to new conditions.

(*c*) Meaning—*i.e.* the associations roused by the names and forms—is the factor most influential in securing successful learning with all the children.

(*d*) Complication in form and difficulty in saying the name result in slow learning.

(*e*) The method of learning acts as an associative factor between names and between forms more than any intrinsic resemblance.

6. Defectives are very slow in correcting errors.

7. They tend to develop automatism in answering.

8. On the whole, defectives show in learning:

(*a*) less power than normals to form associations except such as are very obvious or are forced upon them from outside;

(*b*) a tendency to be confused rather than helped by chance associations that arise;

(*c*) less power of criticizing their results;

(*d*) less ability to work under new conditions;

(*e*) a tendency to automatism.

They fail to retain chiefly because they fail to associate; they re produce wrongly because they fail in judgment.

EXPERIMENTS ON THE PROBLEM OF MIRROR-WRITING

BY LUCY G. FILDES.

Experiment 1 (pp. 57–59); *Experiment* 2 (pp. 59, 60); *Experiment* 3 (pp. 60–62); *Experiment* 4 (pp. 62, 63); *Experiment* 5 (pp. 63, 64); *Experiment* 6 (pp. 64, 65); *Summary and Conclusions* (pp. 66, 67).

1. Mirror- or reversed writing is very commonly found among young children and among defective children. It varies in kind from the occasional reversal of single letters to a complete reversal of all words and letters written (true mirror-writing). The latter, however, is comparatively rare.

2. It is found most frequently in association with left-handedness, but it also occurs in varying degrees among right-handed children who have never written except with the right hand.

3. It occurs both in copying letters and in writing from memory.

There seems no doubt that the tendency to reverse letters seen in mirror-writing is only part of a larger tendency, especially among young children, to reproduce forms without any apparent heed to the position which they occupy in space. A form may be reproduced in any position and still be regarded as the same form, as indeed it is. Before explaining the more particular tendency, then (*i.e.* that seen in reversed writing) some reason must be found for the more general one which leads to the neglect of the exact orientation of any form, while the form itself is otherwise well reproduced. The experimental work about to be described[1] attempts a solution of both problems.

EXPERIMENT 1.

This was experiment 1 of the preceding paper (see pp. 40–48). After the forms were known, the subjects were required to draw them with the left hand.

Finally, all the results were analysed from the point of view of studying variations in the placing of the forms.

Results.

The drawings, whether made with the right or with the left hand, which showed clearly wrong orientation were classified into four groups, viz.—

1. Forms with right-left reversal.

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[1] Cf. C. S. Myers and L. G. Fildes, "Left-handedness and the Reversal of Letters." This *Journal*, 1921, XII. 273–278.

2. Forms with top-bottom inversion.
3. Forms with faulty orientation of certain parts only.
4. Forms wrongly placed but giving neither complete reversal nor complete inversion.

Only the well-marked cases of faulty orientation were selected. Many others, perhaps most of the drawings, showed some slight fault in placing. The left-hand drawings were made once only by each boy, the right-hand ones many times.

A summary of the results is given in Table I. The numbers in brackets refer to the above-named types of errors.

Table I.

	Defectives		Normals	
Name of form	R. hand No. and type of errors	L. hand No. and type of errors	R. hand No. and type of errors	L. hand No. and type of errors
Ek	13 (2)		14 (2)	1 (2)
	14 (3)	1 (3)	2 (3)	
	7 (4)	1 (4)		
Nop	20 (1)		9 (1)	1 (1)
	11 (2)		3 (2)	2 (2)
	109 (4)	13 (4)	3 (4)	
Dak	6 (2)		1 (2)	1 (2)
	3 (3)		2 (3)	1 (3)
	4 (4)	2 (4)	19 (4)	2 (4)
Lut	11 (1)	8 (1)	6 (1)	2 (1)
	3 (2)			
	1 (3)			1 (3)
	6 (4)			
Itta	3 (3)	1 (1)	1 (2)	
	2 (4)			
Ena	7 (2)	8 (2)	1 (1)	1 (2)
	1 (3)			
	79 (4)			
Sor	3 (2)	4 (2)	3 (2)	
	10 (4)			
Na	1 (3)			
Total	31 (1)		16 (1)	
	43 (2)		22 (2)	
	23 (3)		4 (3)	
	217 (4)		22 (4)	
	=314	38	=64	11

Note. Of the 38 wrongly orientated left-hand drawings made by the defective boys all but 10 are repetitions of right-hand errors. Of these 10, 7 show right-left, and 3 top-bottom change.

Of the 11 similar drawings given by the normals, all but 4 are repetitions of right-hand errors. Of these 3 show top-bottom and 1 right-left change.

Analysis of the errors suggests that they may be due to one or more of several causes, of which the chief appear to be:

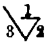
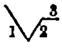
1. Confusion with another form.

2. Assimilation to a similar form.

'Ek' was learnt as 'like a square' and so reproduced as a square.

'Nop' was 'like p' and reproduced as p, etc.

3. Failure to observe the relation of the parts of a drawing to each other. This led to reversal or inversion of the forms.

4. The method of learning, *e.g.*  for  (lines counted and drawn in the order indicated).

5. Repetition of an error previously made.

This was the commonest cause, especially among the defectives. Looking again at a form which had been wrongly reproduced, or retracing it, had as a rule, no effect on their later reproductions. In the absence of external comment the boys tended to reproduce their own previous drawings.

6. The use of the left hand.

This caused errors in comparatively few cases, but it certainly accounted for some mistakes.

Hence,

(*a*) The causes of wrong orientation are many and are by no means confined to the use of the left hand.

(*b*) Both defectives and normals tend to make errors in orientation, though the tendency is greater among the defectives.

(*c*) The main cause suggested by this experiment for continued error among defectives is their tendency to repeat a mistake once made in spite of chances of correction. A further cause probably lies in poor initial observation.

EXPERIMENT 2.

This was experiment 2 of the previous paper. As in Experiment 1 left-hand drawings were made when the forms were known. On the whole, these forms had been drawn far less frequently with the right hand than had those in Experiment 1. The same classification of errors is used as in experiment 1.

Results.

Table II.

	Defectives			Normals		
Name of forms	R. hand No. and type of errors	Possible cause	L. hand No. and type of errors	R. hand No. and type of errors	Possible cause	L. hand No. and type of errors
Ral	21 (1)	Meaning 'question mark'	7 (1) 5 = copies of R.H. er.	6 (1)	Meaning 'question mark'	4 (1), 2 from R.H.
	1 (2) 1 (3)					
Et	3 (3)	Faulty obs.		1 (3) 1 (4)	Meaning 'K'	1 (4) from R.H.
Asa	34 (3)	Faulty obs. and rep. of er.	7 (3) as R.H.	8 (3)	Faulty obs.	5 (3), 4 as R.H.
	1 (4)		1 (4) as R.H.	1 (4)		
Mot	8 (2)	Meaning 'W'	5 (2) as R.H.	12 (2)	Meaning 'W'	7 (2), 6 as R.H.
	1 (3) 11 (4)					
Tup	18 (3)	Meaning 'Z'	1 (3) as R.H.	3 (3)	Meaning 'Z'	5 (3), 2 as R.H.
Ik			1 (1) 1 (2)			
Ret	33 (3) 6 (4)	Faulty obs. Confusion with 'Tup'	8 (3), 5 as R.H. 1 (4)	12 (3)		4 (3), 3 as R.H.
Issi	3 (1) 5 (3) 41 (4)	Confusion and meaning scout mark	1 (3) 11 (4) as R.H.	23 (4)	Meaning scout mark	14 (4) as R.H.
Total	186		44 8 only due to L.H.	67		40 10 due to L.H.

No relation can be traced between the method of learning and the tendency to reversal.

Hence the results entirely support the conclusions drawn from the results of the preceding experiment.

EXPERIMENT 3.

The purpose of this experiment was:

(*a*) To gain further information with regard to the causes of faulty orientation, and

(*b*) Especially to see whether any difference in this respect was shown between forms learnt visually and forms learnt on a motor method.

Procedure.

The material consisted of eight forms—four learnt visually and reproduced seeing, and four learnt and reproduced blindfold, *i.e.* by movement only. The forms were similar to those used in the earlier experiments. The subjects were 25 defective boys.

Each form was shown to, or traced with the right hand by, the subject three times (5 secs. exposure).

A right-hand reproduction was attempted.

If this failed, learning and reproduction were repeated until the form was known; if it succeeded

A left-hand reproduction was attempted.

Results.

These are given in Table III.

Table III.

Type of error		Right hand				Left hand			
		1	2	3	4	1	2	3	4
Visual	Form 1.	.	.	.	.	3	.	.	.
	„ 2.	.	.	.	3	3	.	.	.
	„ 3.	3	.	13 (N)	.	3	.	2	.
	„ 4.	2	.	13 (L)	3	.	.	3 (L)	2
Total		5	.	26	6	9	.	5	2
Motor	Form 1.	.	.	3	1	.	.	.	.
	„ 2.	1	.	.	.	10	.	.	.
	„ 3.	2	.	17	.	1	2	1	.
	„ 4.	10	.	10	.	5	.	.	.
Total		13	.	30	1	16	2	1	.

Note. As before, 1. = RL reversal; 2. = TB inversion; 3. = Wrong orientation of some part only; 4. = Other wrong orientation.

Sources of Errors.

Visual. Twenty-nine errors appear clearly due, in the first instance, to confusion of the form given with similar familiar forms.

Nine appear due to the use of the left hand in particular.

Motor. Thirty-one errors appear due to confusion with familiar forms.

Fifteen are due to the use of the left hand. Of these ten are of Form 1 which consisted of a simple spiral.

Hence

(*a*) No clear evidence is given that any method of learning in particular results in errors of orientation.

(*b*) The factors found operative in producing changes in orientation in the earlier experiments are operative here also, especially:

(i) Confusion with familiar forms,
(ii) Repetition of errors once made,
(iii) Use of left hand as an especial cause of right-left reversal.

(*c*) Certain forms (mainly those having curves) are more subject to right-left reversal with both right and left hands than are others, apart from the factor of meaning.

EXPERIMENT 4.

To get further information especially with regard to the influence of learning by movement on faulty orientation, another series of forms were taught.

These were learnt by ten tracings and then reproduced either by

(*a*) both hands together,

or (*b*) first one hand and then the other.

The subjects were 25 defective boys.

Results.

Form 1. Learnt by ten repetitions with the right hand, blindfold. Reproduced with both hands together.

Wrong orientation in reproduction (*classification as before*).

Right hand	Left hand
3 (1)	16 (1)

I.e. in 19 cases the left hand showed reversal of the right-hand movement made in reproduction.

Form 2. Learnt with the left hand, blindfold. Reproduced with both hands together.

Wrong orientation in reproduction.

Right hand	Left hand
12 (1)	2 (1)

I.e. in 14 cases right hand showed reversal of the left-hand movement made in reproduction.

Form 3. Learnt blindfold right hand. Reproduced first right hand, second left hand, blindfold.

Wrong orientation in reproduction.

Right hand	Left hand
1 (1)	11 (1)

I.e. in 10 cases left hand showed reversal of the right-hand movement made in reproduction.

Form 4. Learnt blindfold left hand. Reproduced first left hand, second right hand, blindfold.

Wrong orientation in reproduction.

Right hand	Left hand
12 (1)	2 (1)

I.e. in 10 cases right hand showed reversal of the left-hand movement made in reproduction.

Forms 5 *and* 6. Learnt seeing, right hand. Reproduced both hands together—5 blindfold, 6 seeing.

Wrong orientation in reproduction.

Form 5		Form 6	
Right hand	Left hand	Right hand	Left hand
0	18 (1)	3 (1)	14 (1)

I.e. in all 30 reversals of the right-hand drawing were made by the left hand.

Forms 7 *and* 8. Learnt seeing, right hand. Reproduced first right hand, second left hand—7, blindfold, and 8, seeing.

Wrong orientation in reproduction.

Form 7		Form 8	
Right hand	Left hand	Right hand	Left hand
0	7 (1)	2 (1)	10 (1)

I.e. in all 14 reversals of the right-hand drawing were made with the left hand.

(*Note.* Form 8 showed many partial reversals with both right and left hands, due apparently to (*a*) meaning, *e.g.* L, (*b*) past experience of similar forms.)

Hence

(*a*) If movement is used for learning a form and is practised, a strong tendency to right-left reversal exists when a reproduction is attempted with the opposite hand.

(*b*) The introduction of a visual factor in learning or in reproduction does not affect this tendency—at least in the case of high-grade defective boys.

(*c*) The tendency is greatest with simultaneous reproduction with both hands. Successive reproduction with the two hands decreases the tendency.

(*d*) Meaning and past experience tend to cause reversal.

(*e*) The tendency to reversal existed to a greater or less degree with all the subjects tested. It may, therefore, be regarded as a common one.

EXPERIMENT 5.

The aim of the experiment was to see whether the tendency to faulty orientation exists in copying forms. The subjects were 25 defective boys (as above).

Fourteen forms, similar to those used in the earlier experiments, were given to the boys to copy. Copies were made (*a*) with the right hand, (*b*) with the left hand.

Results.

Right-hand drawing: 34 cases of wrong orientation occurred, *i.e.*

10 (1), 2 (2), 22 (3).

Left-hand drawing: 27 cases occurred, *i.e.*

11 (1), 3 (2), 11 (3), 2 (4).

Of these all but 11 were repetitions of right-hand errors.

Hence

(*a*) A tendency to error exists with these subjects in copying forms.

(*b*) This tendency is as great with right- as with left-hand copying —even including cases of right-left reversal.

(*c*) An error made with one hand is likely to be repeated by the other.

(*d*) Errors seem chiefly due to confusion of the forms to be drawn with other similar known forms.

(*e*) Reversals due to the use of the left hand were few. Probably this was because the forms were not repeated by the right hand as they were in the preceding experiment.

EXPERIMENT 6.

This final experiment on orientation had as its object the comparison of the writing of defective boys with that of normal children of about the same mental age and at approximately the same stage in learning to write.

All could copy the alphabet and figures, and could write them in sequence from memory with a fair degree of accuracy; but none of them had had much practice in reading or writing; *i.e.* the letters and figures could not be produced with the automatic accuracy of complete familiarity.

The subjects were 25 defective boys and 32 normal children between 6 and 7 years of age.

Procedure.

The subjects were required:

1. To copy the alphabet and figures 1 to 9 from printed slips.
2. To write the alphabet and figures from memory on blank slips.
3. To write the alphabet and figures from memory on papers on which these were already written but were upside down in relation to the child, *e.g.*

(*a*) ⇂ ᘔ Ɛ ㄣ ˙ɔʇǝ (*c*) ɐ q ɔ p ˙ɔʇǝ

(*b*) ˙ɔʇǝ ㄣ Ɛ ᘔ ⇂ (*d*) z ʎ x ʍ ˙ɔʇǝ

In the case of 3, the children were told to write all the letters exactly as they had done before.

Results.

A summary of the results is given in Table IV.

Table IV.

(The numbers indicate the number of faults in orientation.)

	Copies	Free rep.	Free rep. under ı 'ᄅ 'Ɛ ˙ɔʇǝ	Free rep. under 6=8 'ㄥ ˙ɔʇǝ
Defectives:				
Figures	1	2	8 and 6′	11 and 8′
Letters	0	11	26 and 7′	31 and 6′
Normals:				
Figures	0	4	58 and 11′	61 and 11′
Letters	1	22	84 and 14′	117 and 50′

Note: (*a*) ′ denotes that the whole of the alphabet or of the figures were copied as shown on the paper slips. It is doubtful whether these children really understood what they had to do.

(*b*) The errors given occur in reproductions in which some letters are right and some are wrong. The wrong letters may, therefore, be assumed to be due to the influence of the copy, as the children knew what they had to do.

(*c*) Of the wrong letters and figures, some (30 of those of the defective boys, 141 of those of the normals) were actual copies of the letters and figures as they appeared to the child. The rest were partial copies only. They were reversed but not inverted.

(*d*) The figures most commonly wrong were 3 and 9. The letters were b, d, g, p, q (129 of these).

Hence

(*a*) Defectives and young children with whom the appearance of a letter or figure is not very strongly associated with its name, tend readily to turn the letters or figures in a wrong direction, especially if any suggestion is offered which may lead them to do so.

(*b*) Certain letters or figures, viz. those nearly alike, are more open to this form of suggestion than are others.

(*c*) It is easy to see how such errors, if uncorrected at first, are repeated and lead to great confusion on the part of the child as to what direction the letter or figure actually should take.

SUMMARY AND CONCLUSIONS.

1. Reversed or 'mirror' writing is only a particular case of a tendency shown by a large proportion of children, *i.e.* the tendency to draw a form in a different position from that in which it is presented.

This tendency may show itself in:

(*a*) Altering the position of the whole form. Apparently the form can be grasped as a whole without a knowledge of its position in space, which is often unobserved unless attention is especially directed to it; or

(*b*) Altering the position of certain parts of the form in relation to certain other parts. There is some evidence that an analytical method of learning (*e.g.* counting the number of lines in a drawing) produces errors of this kind in particular.

2. Errors in orientation occur both in copying forms and in reproducing them from memory.

3. The main causes of such errors are:

(*a*) Confusion of the form to be learnt with another form which is either (1) familiar, or (2) presented at the same time.

In such cases the form is often not clearly perceived because it is regarded as already known.

(*b*) Weak association of the form with a name.

4. The type of learning, whether visual or motor, does not affect the tendency to faulty orientation except in the cases indicated below. Further, except under certain circumstances, errors are equally common whichever hand is used in reproduction.

5. The particular error in orientation seen in mirror-writing—right-left reversal of forms—is especially associated with the use of the left hand in cases in which:

(*a*) A right hand movement is reproduced simultaneously with the two hands.

(*b*) A movement, learnt and practised with the right hand is reproduced with the left.

6. The particular association of mirror-writing with defect depends apparently on the following factors:

(*a*) Many defective children never get beyond the stage of incomplete familiarity with letters—a stage at which, as has been shown, mistakes in orientation are easily made.

(*b*) Defective children on the whole tend to repeat without further criticism what they have once done. They do not, therefore, correct accidental mistakes in writing and so become confused.

(*c*) A fairly large percentage of defective children are left-handed. They are therefore more liable to make the mistake of right-left reversal of letters, especially when a motor method of teaching is employed, or when writing is attempted first with the right hand. Because of the absence of criticism on the part of the children, such mistakes, once made, tend to be repeated.

7. It must be remembered that the association of a letter name with the particular placing of a special form is a purely arbitrary one. If N Z Ƨ N are all called 'zed' at first, there will be no difficulty in reading or writing any or all of them as 'zed' later; the particular association of 'zed' with 'Z' will only come after much practice, especially practice in reading, of which most defectives do very little.

(*Manuscript received* 6 *May*, 1922)

COMMUNICATION *VERSUS* EXPRESSION IN ART[1]

By LASCELLES ABERCROMBIE.

A GOOD deal of the recent interest in æsthetics must be due to Benedetto Croce; for by his restatement of the æsthetic problem, he has restored it to the condition of being *arguable*. For theories of art which began by evoking such unruly mysteries as *beauty* and *idealisation*—theories which then found that, instead of being guided by friendly powers, they had started on a series of battles with invisible demons—for these confusions Croce substituted a simple tactical movement, and quietly showed logic the way in: the theory of art became the logic of expression. This was so refreshing that it was hailed as a revolution: and Benedetto was indeed the right name for its author. Historically, as he himself might say, it was certainly the readjustment required; but of course it was not new, and he would be the last person to claim that it was final. A further readjustment has been suggested, and this again will prove to be nothing new; for to make, as strictly as Croce does, æsthetic theory the theory of expression has revived the question whether the *theory of art* should not be a limitation of general æsthetic theory—whether, that is to say, art does not require a closer specification than *expression*. Ought not, perhaps, the *theory of art* to be some theory of *communication*? (To the implied distinction between art and æsthetic I shall come later.)

Those who think (I am one myself) that this would take the theory of art a step beyond Croce, may usefully be reminded that it is a step which Aristotle took almost without noticing it, as a matter of course. But the importance and significance of a move forward in philosophy is not so much in arrival as in departure: not so much in the place we get to as in the direction we have come, and why we have come.

The first and, for some things, still the best treatise on these studies also approached art from the side of expression. It was natural for Aristotle, as a Greek, to assume that art must be, in some way, a *mimesis*. The notorious dangers of translation have allowed this technical term of his to obscure one of his greatest services to æsthetics. But he is himself to blame, for he does not always use the word in the same sense. *Mimesis* is, I suppose, sufficiently translated, as it is commonly used in Greek, by 'imitation'; and there are some passages in the Poetics in which this is right enough. They are all passages which not only are unnecessary to the main structure of his theory, but which flagrantly contradict it. He is not the only philosopher who has tried to extend his argument by slightly dislocating the meaning of his terms. For outside these occasional and anomalous passages, the word *mimesis* is always used by Aristotle in a special connexion, which imposes on the word a special sense: a sense which our word 'imitation' is not accustomed to bear. Thus, if we translate literally (that is, with a false rigidity of verbal equivalence, and therefore incorrectly), Aristotle is made to say that tragedy 'imitates' a certain kind of action: and the whole process of his argument shows this action to be something that cannot actually occur: it can only occur as an idea. Clearly, this is the sort of imitation which he postulates in music and dancing; it is, in fact, not what we call imitation at all: it is expression. Indeed, to translate literally sometimes makes Aristotle's *mimesis* yield mere nonsense: as when poetry "imitates men better than they are." He must mean formalised or idealised imitation, say some commentators: that is, imitation which is precisely not imitation. Others say, he is simply thinking of the heroes of legend; but how can poetry imitate men who are not there to be imitated? Some of Aristotle's followers have encouraged this confusion by sticking to what they conceived to be his terminology, though they saw clearly enough what he meant. Thus Sidney, with no intention of startling us, says the sacred poets "imitate the inconceivable excellencies of God"; and in a passage which crystallises the real sense and the apparent paradox of what I may call Aristotelian imitation, Sidney contrasts "the meaner sort of painters, who counterfeit only such faces as are set before them," with "the more excellent," whose work he typifies by a picture of Lucretia, wherein the artist "painteth not Lucretia whom he never saw, but painteth the outward beauty of such a nature"; this, he says, is to imitate, but adds that such an artist, in order to imitate, "borrows nothing of what is, hath been or shall be." Sidney does not mean that the artist does not borrow the elements of his technique from nature; his argument has sufficiently provided against the imputation of such absurdity. He means that the finished work as a whole imitates something which does not exist in nature. Where does it exist? It exists ideally, until it is imitated in art—that is, expressed.

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[1] Read to the Æsthetics Section of the British Psychological Society, January 29, 1923. The paper supposes some acquaintance with the arguments so brilliantly sustained in Croce's *Estetica come Scienza dell' Espressione* and *Breviario di Estetica*.

I elaborate the Aristotelian meaning of imitation in art, not merely because that meaning is expression, but also just because it was on this very word, the word for imitation, that this special sense was imposed. Plato had taken the usual line that art *begins* by imitating objects. Aristotle's argument is not really concerned with how art begins. The chapter in which he perfunctorily accepts, with Plato, the commonplaces about art's origin in mimicry is quite unnecessary as a starting-off place for his argument, quite at variance with his main thesis, and quite untrue. It is a mistake which we may legitimately ignore. It is indifferent to Aristotle's theory how the artistic impulse arises; though for one art—tragedy—he describes keenly and with profound insight the form the impulse must take in order to be expressed. The first importance of his argument is that it brings in *mimesis* at a *later stage* of the genesis of a work of art. *Mimesis,* for Aristotle, comes when the artist has to find, in the medium he is using, an equivalent to his impulse, his idea, his intention, etc. This, once stated, was decisive, both against Plato and for the true theory of art. Plato called the *conception* of a work of art a *mimesis*: Aristotle called the *technique* of a work of art a *mimesis*. That is to say, out of the vague popular notion that art is imitation, which Plato made into ingenious nonsense, Aristotle drew the essential doctrine that art is expression.

But he still called it imitation! What justification can there be for that? A very good one, if language would allow it, as Greek apparently did but English hardly can. For by speaking of artistic expression as imitation, Aristotle makes it impossible for us to forget that artistic expression must be *recognisable*—that is, universally recognisable. It must not only be good as expression—expression in which the impulse is satisfied: it must be good in the opposite direction—as revelation of the impulse. To call expression in art imitation was Aristotle's way of implying that expression in art is also communication.

No doubt he thought it enough to imply a thing so obvious; but his theory as a whole certainly required that it should at least be implied. For Aristotle also assumed that art has a function, which he described as *katharsis*. His whole theory may be summed up as *katharsis* by means of *mimesis*—expression which necessarily has a function. Now if art has a function it cannot be confined to the artist. It cannot be a function which *stops short*; for to speak of the function of art is to speak of its peculiar effective relation to life in general. And whatever we may think of *katharsis* as an adequate or even intelligible account of art's function, it at least applies equally to the artist and to his audience. Aristotle indeed hardly attended to the function of art in the artist; and perhaps no one did until Goethe took it so seriously. What interested Aristotle was the function of art in the artist's audience; and this, he says, takes place by means of expression. An artist creates by expression a work of art, and this effects in its audience that for which it had throughout been designed. What else can this mean but that the expression which creates a work of art is an expression which is necessarily also communication?

If so, art is an expression of the same kind as language; and it is in fact an almost irresistible metaphor that an artist, whatever his medium, *says* something. Now a man who speaks evidently expresses himself; but it is an expression which would not be language unless it were communicable. It does not exist, of course, in the signs and sounds by which it is communicated. It does not exist in the air or on paper, in our mouths or our ears or our eyes. It exists, that is to say, as expression; and just so art is by many said to exist. But it is equally true that language does not exist *merely* as meaning; for to speak of language is to speak of meaning *as conveyed* by signs and sounds. The idea of language is not simply an idea of expression, but of expression which can pass from one mind to another. Language in the speaker's mind is expression, and in the hearer's mind it is expression. The characteristic thing about language is the further fact that the second is the result of the first: that language is communication. Here too the analogy with art seems to hold.

But there is an important refinement in the idea of language which requires to be brought out in order that we may be safe with this analogy. Dante puts it in a compact sentence which, I dare say, many people pass over as a mediocre platitude or a medieval oddity. "It is more human," says Dante, "to be heard than to hear. *In homine sentiri humanius credimus quam sentire.*" That is to say, the nature of language is more concerned with the speaker than with the hearer.

There is first a speaker: on that bare fact we shall all agree. Now, it may be argued, language is for the speaker simply expression; but to the hearer it is communicated expression. Since, therefore, the speaker comes first, language as expression must be ranked above language as communication. And must not this hold good of art too? Must we not say that art is essentially expressive, and only by accident communication? May it not even be (as some seem to think) that communication in art is no more than an epiphenomenon, the presence or absence of which is wholly indifferent to the real fact and essential nature of art—the real fact being simply an intuition, which cannot occur except as an expression? This of course would make an experience of art quite incapable of being differentiated from an experience of natural beauty: a conclusion which many not only seem to accept but to welcome.

But what does Dante's characteristically terse and searching remark really mean? For his whole discussion is based on the obvious axiom that language exists as communication or it does not exist at all: the speaker *requires* a hearer. But if a man communicates something to us, he communicates something that is *his own*—something he has made his own; and we accept it as his. Expression is supposed by, and included in, communication. But what expression? Language, says Dante, exists rather by *being heard* than by *hearing*; and this significant antithesis of passive and active means just this: that *another man's expression* is being made to occur in us *as our expression*: the thing of first importance which language effects is not so much that *I understand* as that *a man is understood by me.* Therein lies the essential nature of communicated expression. For all expression is personal; and conscious experience is nothing but personal expression. What I experience I personalise. Now if I experience nature (let me use that convenient word), in that act something becomes the merely passive to the active of my expression; and there is no personalisation in the experience except mine. But in communicated experience, what is given to me is already personalised; what stands as the passive to my active was previously active itself: another spirit has been before me. And I know it. When an expression is communicated to me, what occurs is my way of expressing what has already existed as an expression, and is so accepted by me. Must I therefore say it is my way of intuiting what has already been intuited? I can at any rate say that I intuit it *as* what has been already intuited; for it is given to me in the form of an intuition: unlike nature, which is given to me wholly and *ab initio* to be formed. It makes no difference to allow that, after all, communicated expression exists not by being communicated, but by being expressed: that is, *in me.* That scarcely needs saying; what does need saying is that communicated expression differs decisively from all other kinds: not by the mechanical fact, but by the spiritual fact, of being communicated.

This is the sense in which I would insist that art is communication against those who will only have it to be expression. I would insist, in fact, that expression in art must be regarded as being also communication, because it must necessarily take that form in order to be what we call art; expression succeeds in art when it succeeds as communication. All objections based on the truism that art exists at last as expression fail to take into account the fact that mere expression does not give us the quality of art; that is only to be found in the spiritual quality of communicated expression. We know that art does not exist in a book or on the stage, in a piano or on canvas: it exists in us. But it exists in us as something communicated, and would not have existed except for its communication. Works of art are made by persons other than ourselves. In the author, art's existence is expression: in us it is the same. But its existence in us is the result of its existence in the author. The mechanical fact of this, however, is important only for the spiritual fact it embodies: the fact that expression in art is not only ours, but the artist's.

"Landscape is art," says Croce. He does not mean a landscape on canvas, but the landscape you see through a window. We must agree with him, if we accept without modification his theory of art as the logic of expression. But suppose I were what is called a landscape-artist: should I merely put on canvas the landscape I *see*, and thus simply fix the *opportunity* for repeating my intuition? By no means: I am reckoned an artist in so far as I manage to fix the intuition itself; and I should do so, not by imitating in paint the landscape seen, but by means of certain qualifications which painters call 'design' or 'composition.' This, to be sure, would be expression. But to what end? How is it judged effective? It is judged effective precisely by its power to compel others to experience what I have experienced; which is assuredly much more than what my eyes have seen. It is effective, that is to say, not merely by its power to express, but (involving that) by its power to communicate; and as it is judged effective, so it is judged as art. When we look at a painted landscape and find it worth looking at, we do not find there what we could have provided ourselves: we do not find simply *a view*; we find there another man's spirit, communicated in terms of landscape, and we find it worth knowing. To equate art with the æsthetic experience of nature, is to give an instance of the familiar process of widening denotation until connotation vanishes. The word loses its meaning; and we must turn to and find another word to label those things which hitherto we have called art. Labelled they must be, for they are different from anything else, and especially different from nature. They differ precisely in being communicated experiences—in the spiritual quality which that implies.

We require two sciences, with two distinctive titles. Adopting words already in use, we require to be quite clear what we mean by æsthetics, and what we mean by the theory of art. Closely related though they are, their subjects should be kept distinct. The general study of experience as such—of intuition and expression—is the large topic of æsthetics.

This, among other things, would propose to account for, or at least, to investigate, our admiration for and delight in nature, with, no doubt, such discriminations of the naturally *sublime* and *beautiful* as would prove necessary. But the theory of art would study the communication of this, —of whatever can occur under the heading of æsthetics, that is, the communication of personal experience; the conditions and means of its communication, and the spiritual significance, not to be paralleled in nature, of communicated experience, with the special qualities (which nothing can replace) of the artistically sublime and beautiful. Let me admit here that the value of art is the value of the artist; but I am not to be manœuvred from this admission into admitting further that I value art simply because I find there in an unusual degree (owing to the artist's superior faculties) the sort of experience I have in nature. The value of art and the value of nature are incommensurable. Only by confusing the topics of æsthetic science and of the theory of art could one be set above the other. No sane man could wish to live wholly in art; and if he did wish it, he could not possibly do it: like Caliban, he must eat his dinner. But we rightly value art for something we can never get in nature: for the satisfaction of some 'desire of the mind' which can only come under the peculiar spiritual condition of communicated experience. The elucidation of this should be the aim of any theory of art.

That art is communication we should, of course, be able to assert on better grounds than the authority of former theories or the analogy of language. So far I have endeavoured to explain precisely what is meant by saying that art is communication. But if I have succeeded in doing so, no more should be needed now than to glance at the fact of art in one or two of its aspects in order to see if communication, in the sense I have described, really does hold good there. It is said, for example, that all people have the experiences of artists. This is not true, for all people have not the experience of making a work of art. But your out-and-out expressionist pays scant attention to that, regarding it as a sort of adjunct, extrinsic to the real fact: if you want it, you have only to want hard enough, and you will get it. But leaving out this most important contribution to the special quality of art—a contribution only to be accounted for in the view that art is communicated expression—let us allow that all people have the sort of experiences which impel artists to make works of art. Does that make everyone an artist? It has been argued that it does; and I find it difficult to regard this as anything better than philosophical sharp practice. But the logic of art as expression requires it. Art is intuition; everyone has intuition; so everyone is an artist. But the man who calls himself an artist and has never made a work of art in his life, is he not a notorious figure of fun? And why? Because the non-practising artist is a contradiction in terms. Let everyone have the same intuitions as artists; nevertheless a person is only called an artist when his intuitions become works of art: when he makes *his* experiences available for *others* to experience. If you do not call him that you must call him something else; for he is an exceptional person, who requires to be classified for the mere convenience of talking about him. Why not keep the word which everywhere means precisely this person? But if you abolish the notion of communication in art, you empty the meaning out of the word artist, and must find another word to contain that meaning: "*the man who can publish his experiences.*"

And he *must* publish them. Until he has published them he is a man frustrated. Now if expression were all he wanted, by the mere fact of *possessing* his experience he has it. But we know that the artist is precisely the man who is not content simply to possess his experience. He [illegible] not content until he has also made it communicable: in a work of art. [illegible] note this also. He must *learn* how to communicate it, and in order [illegible]ceed he must devote himself to labour at it. Justly, if art is com[illegible]tion, we speak of a *work* of art: the phrase is a superfluous com[illegible], if expression by itself (that is, the happy event of an intuition) will account for art. Either Beethoven and Milton, in their prolonged struggles to perfect their art, were wasting their time; or their furious industry was *needed*: and in that case their art is not to be accounted for simply as expression. It was the communication of expression they were labouring to perfect. For to do so, they had to translate intuition into the medium of their art—into the instrument which could communicate it; since intuitions themselves are clearly incommunicable. They had, in the Aristotelian sense, to *imitate their experience*: the word well expresses the process of making one set of things *represent* another—an essential part of the fact of art. It needs no elaboration, and the expressionist theory can only evade it. If a poet wishes to kiss a girl, he may do so and write a poem about it; or not do so and write a poem about that. The girl may be real or imaginary; but in any case the affair, which ever way it went, did not take place in words. As æsthetic man, the experience itself would suffice him. As poet, it would not; he would have to find the words for it—the words which, intuited by us, would make his experience ours. Then art—poetry—would have occurred; expression would have taken the form of communication.

This process, which is nothing but the *technique* of art, and may be summed up as symbolic representation, holds good everywhere: not more in music and poetry than in those arts, such as landscape painting, which are erroneously supposed to be in some special way representative. All art is representative of spirit: no art is representative of nature. For the thing submitted to us to be experienced in art is already in the form of an experience, completely organised and isolated into coherent unity of expression. But that expression, which is art, occurs in us because we have translated it out of the artist's *technique* of communication, and could not occur unless it had been so communicated: that fact gives it its special quality as art. The law which governs artistic technique is obvious and universal. It is that the effective is also the expressive—or rather the communicative, remembering that we take communication to include expression. If A can be relied on to effect B, then clearly in order to communicate B we can use A. This shows the importance of those studies in æsthetic psychology which investigate, to the amusement of the expressionists, our reactions to certain shapes, colours, tones, etc. It shows, for example, what Lipps has contributed to the bett understanding of art. Such studies will never give us the nature of but they will give us the means available to the *technique* of art—wha artist can rely on for the communication of his experience.

Let me correct beforehand a possible misinterpretation. *technique* symbolic representation; and the phrase must be taken That is to say, there is not first the communication, and then the ex sion in our minds: the communication does not unburden itself message, which we then appreciate; we do not have to extricate a mea ing from *technique*. Art (whatever emphasis we may lay here or there for polemical purposes) is not to be regarded as communication effecting expression: but rigidly and always as *communicated expression*—expression which cannot occur except as and when communicated. There is no question of *technique* being fitted on to content, as there might be if it were the mechanical fact of communication which mattered. What matters is the spiritual fact of communication: that the expression which is art cannot occur in us except as communicated by sensible *technique*—which cannot but mean, in the form already of a perfected experience: not requiring us to make (as in our everyday life) our usual more or less successful spiritual conquest of nature, but requiring our own personalisation of a conquest already won. The assurance of significance which gives to art its irreplaceable and inestimable value is to be derived from this very fact. But to discuss that would take me too far from my immediate topic. I mention it to show how vital, in my view, the idea of communication is to the true theory of art.

Two common objections may be briefly met here. If art is communication, how does it differ from other communications—*e.g.* language? This is not serious. Art communicates not instruction, information, argument, persuasion: it communicates experience. And it communicates this whole: not the object of it nor opinion about it nor feeling for it, but experience itself, which includes all these. Hence the many-sidedness of artistic *technique*; and hence its formality. For experience, an incommunicable unity, must disintegrate as it passes into its complex communicable representation in *technique*. But this must, in every moment of it, provide for a final complete re-integration into artistic *form*, which by exhibiting itself as self-contained answers to the original unity.

But if an artist labours to make his expression communicable, must that not mean that he composes for his audience, not for himself? I have very little patience with the notion that Shakespeare put in his vulgar passages in order to tickle the mob. I personally enjoy these passages, and I naturally assume that Shakespeare, though he knew they would 'go down,' put them in because he liked them. But the objection is simply a misunderstanding, which may be cleared up on the analogy of language as Dante's trenchant phrase sums up its nature. Language exists primarily not because men want to hear but because men want to be heard. The motive comes from the speaker, but is directed to the hearer. Speech does not exist for the sake of the hearer, but it is necessarily accommodated to the hearer. If a man has something to say, he takes care to say it intelligibly; for otherwise he does not say it at all. So art does not exist for the sake of the audience; but it does not exist at all if the audience cannot take it in. Art is governed by the requirement of the artist; but he would not be an artist unless he required to be understood. His expression, as an artist, necessarily takes the form of communication. And the more successful his communication is as an appeal to his audience, the more he has his audience at his mercy—the more they must submit to *his* experience. The supreme artists are the supreme masters of communication.

INSTINCT, EMOTION, AND THE HIGHER LIFE

By LOUIS ARNAUD REID.

- I. (*a*) *The view that instincts are the prime movers of all human activity* (pp. 78–79).
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    - (*d*) *This applied, speculatively, to evolution in general* (pp. 92–93).

I

(*a*) It is my intention to examine in the following paper the contention very commonly made to-day that instincts are the sole driving forces of all human activity, and that what we commonly call the 'higher' life of man must in the end be expressible in terms of these innate specific impulses. In doing this I shall be concerned mainly with the kind of doctrine first set forth by McDougall in 1908, a doctrine which although by no means accepted in its entirety by contemporary psychologists, has yet had a very striking influence upon the thought of very many of them.

It is unnecessary in this *Journal* to restate McDougall's own views, except in a sentence or two. His familiar contention is, in a few words, that the whole complex system which we call the 'higher' or moral life of man is ultimately explainable by reference to two main factors, acting and interacting, namely those specific innate tendencies to be found in man which he has in common with the higher animals, and the influence of environment, including self-consciously known objects and the moulding process which we call education, upon these innate tendencies. "Directly or indirectly," says McDougall, "the instincts are the prime movers of all human activity; by the conative or impulsive force of some instinct (or of some habit derived from an instinct) every train of thought, however cold and passionless it may seem, is borne along towards its end.... All the complex intellectual apparatus of the most highly developed mind is but a means towards these ends...." In these impulses "we are confronted with the central mystery of life and mind and will[1]." The emotions and the emotional dispositions accompanying these instinctive impulses are, according to Shand's now generally accepted theory, organized in human beings in systems, called sentiments, about various objects. These in their turn must, if 'moral character' is to come to its full fruition, be organised into some "harmonious system or hierarchy[2]." But however complex a thing ripe moral character may be, it is in the end simply and solely a product of instinct and environment. The term 'environment' must, as was said, be taken in a broad sense, and the organization of instincts through sentiments into character is "rendered possible only by the absorption of the more refined parts of the moral tradition, under the influence of some of the personalities in whom it is most strongly embodied. These persons...exert this influence upon us in virtue principally of the admiration that they evoke in us[3]." When we turn to the psychology of the moral leader himself, we find that his character is organized in the same way as his fellows, but that in his case the innate tendencies are particularly vigorous. What McDougall calls "original moral judgments" come directly from sentiments based upon emotions which are themselves but the affective sides of instincts. It is therefore in the last resort simply and solely the *strength* of instincts and their attached emotions, which determines 'original' moral character and judgments, though we must not of course forget the influence of environment.

(*b*) Although it is certain that the foregoing statements cannot be refuted by a bare denial, we now have to ask whether they are exclusively true. Are *all* the facts before us, is the moral life *wholly* explainable in terms of instinct and environment, is the capability for moral leadership in a man *solely* in the end a question of his predetermined native dispositions? Or is there not at the human level something more than this, something left out of the theory of 'instinct psychology,' but which nevertheless is not to be discovered by some easy reference to 'moral reason,' 'conscience,' or a mysterious 'faculty'? If this is so, then it will follow that, however deeply (more deeply than he is ever directly conscious of) man is the product of animal evolution, he is not only determined by that as it is usually conceived, but by something different as well, and that moral development will not be explainable in terms of strength of instinct.

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[1] *Introduction to Social Psychology*, 44.

[2] *Ibid.* 259.

[3] *Ibid.* 228.

(*c*) To prove this is not altogether easy. We must (i) examine views of the relation of instinct to emotion, in order to find out whether there is, as McDougall contends, any direct and necessary connexion between them, and whether emotion is necessarily instinctive. If we can find that there is no such necessary connexion, we may (ii) after investigating more particularly the nature of emotion itself, be able (iii) to establish positively certain emotions and conations important for the 'higher' life which are not primarily instinctive, however true it may be that in their evolution instincts have played a part. Afterwards we shall discuss certain implications of the views put forward.

II

(*a*) McDougall defines instinct as "an inherited or innate psychophysical disposition which determines its possessor to perceive, and to pay attention to, objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or, at least, to experience an impulse to such action[1]." The emotion is for McDougall simply the affective element in instinct.

This view of McDougall has been criticized by Drever[2] (who in many respects accepts McDougall's position) under two headings: (i) Does the term 'emotion' properly apply to the affective element in every instinct experience? and (ii) Does not McDougall's view artificially exclude a number of experiences (*e.g.* surprise) which we should in the normal course of events term emotional? The first part of the question Drever answers by substituting (very justly, I think) the term 'interest' for

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[1] *Introduction to Social Psychology*, 29.

[2] *Instinct in Man*, 156–161.

'emotion' as the affective element in instinct experience, and contends that it is only when satisfaction of interest is checked, and interest passes into 'tension' that emotion arises. The biological function of emotion is to reinforce interest and impulse. A very similar thesis has been advanced by Carver[1]. With the second criticism (ii) we shall also find reason to agree.

(*b*) Although I shall not in this paper be concerned mainly with instinct, I ought perhaps to indicate (as we are to discuss the relation of instinct to emotion) the sense in which I shall use the term 'instinct.' Psychologically, McDougall's account of instinct as an innate disposition seems sound enough, although apart from the functioning of these innate dispositions in 'instinctive' actions we should know nothing of them. 'Dispositions' indeed, by their very definition, must be for us hypothetical abstractions. If we forget this we may be led into the mistakes of 'faculty' psychology. Actually in any given case instinct appears to be accompanied by intelligence, as is intelligence accompanied by instinct. It is common to members of the same species, it tends towards preservation, and instincts are shared by man with the higher animals. Again, it appears to be the presence of some degree of intelligent consciousness[2] which distinguishes instinctive from reflex action, and it is the capability of devising means to ends and of learning by experience which characterizes intelligence. Instinctive dispositions are, I suppose, to be distinguished from congenital dispositions not usually called instinctive (*e.g.* capacity for music or any special ability) in that they are common to the species. But general statements on this point are of doubtful value, owing to the lack of knowledge in this field. McDougall, who would agree with this, suggests[3], "as a speculative hypothesis," that instincts have the nervous and bodily structures through which they operate innately laid down, whereas innate endowments for such things as music have no such given bodily organs. This however seems to me so "speculative" as to be valueless: in the lack of physiological evidence it is at present impossible to prove, and as an hypothesis it sounds improbable. But more important than this objection is a more general criticism which might be levelled against McDougall. Why, if instincts in man are but a *part* of a larger group of innate dispositions, are we confronted in them with "the *central* mystery of life and mind and will"? An important part as they may be, they cannot, out of McDougall's own mouth, explain all. The excellent plan employed in the classification of the instincts, of marking the chief instincts as those shared by man with the higher animals, can be carried too far if it is supposed, on any occasion, that the *chief* innate dispositions are *all* the innate dispositions.

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[1] "The Generation and Control of Emotion," This *Journal*, 1919–20.

[2] See Stout, Symposium on "Instinct and Intelligence," This *Journal*, 1909–1910, 244.

[3] Symposium on "Instinct and the Unconscious," *ibid.* 1919–1920, 38.

(c) We must now examine more closely the relation between instinct and emotion. Is it *true* to say, as McDougall does, that emotion is the affective side of instinct, and does it throw any real light on the nature of emotions? It has already been pointed out elsewhere[1] that we have no direct knowledge of the affective sides of instincts in animals.

Let us take a concrete example. McDougall says, speaking of Admiration: "This is certainly a true emotion, and it is as certainly not primary[2]." Why is this "certainly" an emotion? Is it *because* it is the complex affective state caused by the fusion of two others (wonder and negative self-feeling) which are themselves the affective sides of instincts; or is McDougall unconsciously referring to some other, perhaps more popular, conception of emotion? If he is, as the quoted phrase seems to imply, he immediately thereafter draws back within the confines of his definition, which he applies with consistent rigidity. We have to ask, however, whether the more usual conception of emotion is not sounder, and McDougall's conception unworkable.

McDougall defends his usage of the term 'emotion' as in the interests of 'scientific' terminology. He says, "in adapting to scientific use a word from popular speech, it is inevitable that some violence should be done to common usage; and, in adapting this rigid definition of emotion we shall have to do such violence in refusing to admit joy, sorrow, and surprise (which are often regarded, even by writers on psychology, as the very types of emotions) to our list whether of simple and primary or of complex emotions[3]." In his analysis of joy and sorrow[4] he finds these to be not emotions, but "complex emotional states...in which one or more of the primary emotions, developed within the system of a strong sentiment, plays an essential part." So again, surprise is not a true emotion but "is merely a condition of general excitement which supervenes upon any totally unexpected and violent mental impressions[5]."

(d) It is highly questionable as to whether scientific terminology gains by such distinctions. They can throw no light upon the relation between instinct and emotion because they beg the question by defining emotion as the affective side of instinct. We may freely admit that emotion is "an affective state with something peculiar about it"; but we clearly ought to *prove* its necessary relation to instinct if statements of such relationship are to stand as valid. Not only must instinct be shown to involve necessarily specific emotion, but specific emotion must be shown to involve necessarily specific instinct. Regarding the former, we have already cited the criticisms of Drever and Carver. The latter McDougall has assumed, not proved, to be true. I shall contend that proof is impossible. McDougall may be right about joy and sorrow (the case of surprise is more doubtful), but there seems to me to be an important class of emotions, usually ignored by psychologists, which cannot reasonably be said to be 'instinctive.' I refer to the kinds of emotion aroused in human beings in the presence, say, of beautiful objects or of a good action or character. I call these (without here defending it) 'value' emotions, because they are aroused in the subject by manifestations of what in metaphysics are known as 'intrinsic values,' *i.e.* goodness, beauty, and truth. In the higher life of man these are very important factors; and however true it may be that instincts (such as curiosity, self-assertion, and self-negation, etc.) precondition them, they are not, I shall contend, themselves instinctive. It will be an important asset from an ethical point of view if we can free certain emotions from a wholly instinctive determination, if we can assume that emotion is broader, deeper, higher, than instinct.

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[1] Drever, *op. cit.*

[2] *Introduction to Social Psychology*, 128.

[3] *Ibid.* 48.

[4] *Ibid.* 151–2.

[5] *Ibid.* 157.

III

(*a*) Without attempting to discuss the respective merits of the various psychological theories of emotion, let us notice one point which many of them have in common. It is the use of words implying that there is some *peculiarity* about each emotion. James says, "There is no limit to the number of possible different emotions which may exist[1]." Stout adds to his general account of emotion "each specific kind of emotion has also something in it *peculiar* and *indefinable*[2]." Ribot speaks of "an agreeable or painful or mixed state of consciousness *peculiar* to each emotion." McDougall says "each of the principal instincts conditions, then, some one kind of emotional excitement whose quality is specific or *peculiar* to it...[3]."

We may accept, without further discussion, that there is something 'peculiar' to each emotion. One of the implications of this assumption which we shall find to be important is that the 'peculiarity' of each emotion sufficiently distinguishes emotion from pleasure or pain, which, although it varies in intensity with each experience, is not usually regarded by psychologists as in itself qualitatively different in each case.

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[1] *Principles of Psychology*, II. 454. [2] *Manual of Psychology*, 311. [3] *Op. cit.* 47.

We shall find further that the 'peculiarity' of each emotion for the psychologist is due to the fact that emotion is not a quality thought of abstractly, like pleasure or pain, but that it is bound up with a whole concrete experience, a complete 'psychosis,' as Ward calls it. Again, emotion is 'peculiar' because the possibilities of concrete experiences are infinite in number. It is because of this that no quite complete classification of emotions which are inextricably bound up with concrete experiences as a whole, is possible.

Although I use the phrase "which are inextricably bound up," as if emotion were a kind of separate entity, I do so, not because I believe that emotions are separate entities, but because it seems to me necessary to distinguish emotion as a feeling from the psychosis of which it is an aspect. When we talk of 'emotion' popularly, we refer to this feeling aspect rather than to the complete state of mind. Following ordinary usage (which in this case is useful) I shall, where I refer to 'emotion' unqualified, mean emotional feeling, the subjective side of a complete experience.

(*b*) I would therefore now define emotion as *the marked psychical affect arising in certain complex situations*. This is not, as a statement, very novel. Baldwin[1], *e.g.* (quoting from Brown's *Philosophy of the Human Mind*, I. 405) says: "Emotions can be defined to be 'vivid feelings, arising immediately from sensations of objects perceived or remembered or imagined, or from other prior emotions'...Bain's usage is equally wide." It is rather in the more precise working out and application of this account in relation to current problems that our definition will prove of value.

(*c*) I shall doubtless be accused of using the terms 'feeling,' 'affect,' 'emotion,' in a loose and undifferentiated sense. In reply I should like to protest against the term 'feeling' or 'affect' being used to denote what ought to be called 'pleasure-pain' or 'hedonic tone.' (Drever, *e.g.*, rejects McDougall's view of emotion as the affective element in instinct because, he says, affective elements can show only two qualities, pleasure or unpleasure.) These latter terms ought, where pleasure or pain is meant, to be used universally, it seems to me, whilst to use 'feeling' to denote pleasure-pain exclusively is to abuse a good word which popularly and psychologically may mean something quite different. However difficult it may be to define feeling, however vague the term may be, it yet connotes something which is definitely understood and directly known, and which is broader in meaning than is 'pleasure-pain.'

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[1] Article, "Emotion," *Dictionary of Philosophy and Psychology.*

Pleasure or pain is a quality, a character, an adjective of all experiences. It is frequently regarded as if it were a kind of psychic entity, apart from these experiences, and to which the term 'degree' can be applied. But this is quite definitely an abstraction, if a useful one. Pleasure-pain is not so much feeling as an attribute of feeling. Hedonic *tone* is really the best word. The acid-alkaline reaction in chemistry is a good analogy. Hedonic tone is to be distinguished from that vaguer, broader aspect of experience (ranging from the experience of physiological stimulus such as touch to the subjective aspects of purely psychical experience) which we call popularly 'feeling.' Pleasure-pain, just because it is so definitely discernible, ought to be clearly marked as such, whilst the remainder of the subjective state should (though no doubt it is pleasurably or painfully toned), just because it is more impossible to disentangle it from cognition and conation, be labelled generically 'feeling.' To be clearer, feeling is the *genus*, of which *e.g.*, the feeling arising from the sensation of roughness, the feeling arising from hunger, (the feeling of) the emotion-psychosis of anger, (the feeling of) certainty or activity[1], are *species*. And all of these are (I here assume) pleasantly or unpleasantly toned. I object to the restriction of the term 'feeling' to this tone. I even doubt, as I said, whether 'tone' can legitimately be called feeling at all, though, being its inseparable adjective, it is of the *order* feeling.

(*d*) It will be remembered that we said "the *marked* affect," and "*certain complex* situations." The words are important, because it is obvious both in itself and from the foregoing that any affect is not an emotion; and not all situations give rise to emotions. Neither the feelings accompanying sensations of roughness nor the feelings of hunger are emotions in themselves. They may, however (I suggest with a certain diffidence), if they are marked and intense, and if they usurp the focus of attention, become emotions, or if this is going too far, they may certainly merge under certain conditions into a complex affective state of consciousness which is distinctively emotional. (It must be admitted, however, that the transition involves change of quality and complexity as well as increase of intensity, and the simple experience becomes greatly transformed.) If we accept the above definition of emotion as satisfactorily describing emotions, then we should say that any feeling when it becomes sufficiently marked, becomes an emotion, whilst if we accept McDougall's theory we should say that any feeling may be the occasioning cause of an emotion.

Let us take an example. To the normal healthy-minded adult the feeling accompanying the sensation of roughness is 'peculiar'; is pleasant or perhaps unpleasant, and that is all. But, supposing the sensation to be intensified, or supposing it to be experienced by a hyper-sensitive child or a morbid adult, then it may clearly form the nucleus of a highly emotional state. The sensitive child, who like all children is absorbed in the present and lacks the mental perspective of the normal adult, may suffer torture from having the finger-tips drawn over a rough surface, such as a stone wall or a carpet, or from being clothed in harsh woollen garments which irritate a sensitive skin. Not only is the sensation painful in feeling-tone and peculiar in feeling, but it may give rise to the severest emotional storms, anger, irritability, disgust, moroseness. The psychological question is,—does the feeling here with its accompanying pain *become* an emotion if it is intensified, or does it evoke an emotion or emotions? McDougall, I said, would give the latter answer; I should prefer the former. There seems to me to be in the situation *more* than anger or disgust or any of the 'instinctive' emotions. There is also the peculiar affect (besides pain) produced by this peculiar 'rough' sensation, with its cluster of associated experiences. This emotion, and every c [illegible] emotion, is unique in quality. Summing up, there is between any [illegible]ng and emotion a difference only of intensity, and of complexity due to the inseparability of feeling from the concrete complex situation. In the one case I as a child slightly 'feel' the roughness, in the other I am violently shaken by a child's emotion.

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[1] I select Ward's examples, *Psychological Principles*, 41.

(*e*) Let us examine briefly the relation and difference between this view of emotion and other views. McDougall, it seems, is right in so far as he attaches emotion to a specific situation, calling it the affective aspect of the complete reaction to the situation[1]. He is wrong, it seems to me, in so far as he attempts to show that *some* list of such situations (he never professes his own to be a complete one) must be exhaustive. No satisfactory list can ever be made, because the number of situations in which feeling may develop to emotional pitch is unlimited. Nevertheless in making this criticism, I do not for one moment desire to deny the value of a classification of instinctive situations. McDougall has done very great service to the cause of psychology and ethics in leading the way towards showing the tremendously important part which instinct and the instinctive emotions play, even in the higher life of man. But it is high time to survey critically the ground traversed: in the 'higher' life of human beings the situations are not so simple and specific, but are infinitely complicated by the entrance of self-consciousness. Emotion is altogether wider than instinct, although no doubt in the zoological world a fairly complete and exceedingly useful catalogue of instinctive situations may be made.

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[1] Though we have already seen that 'interest' may often be more descriptive than 'emotion.'

Again, our theory of emotion accounts for those emotions, usually accepted as such by psychologists and plain men, which McDougall, on account of his rigid adherence to the instinctive view, is obliged "in the interests of science," to exclude. McDougall says, "shyness and modesty...are qualities of character and of conduct arising out of the possession of instincts and sentiments, while shame is a true secondary emotion[1]." From McDougall's point of view this is no doubt consistent; from mine it is an artificial distinction. From my point of view shyness and modesty may be, if as experiences they are at all intense, just as much emotions as shame or anger.

Further, the nature of emotion as such is not affected by any relation [illegible] have to the sentiments. If emotion is as I have said it is, then as [illegible] experience it remains emotion whether there remains an organized disposition to experience that emotion or not. To take a concrete example, there is no reason, as McDougall seems to think[1], why 'love' and 'hate' should *not* be used to designate both emotions and sentiments, so long as we do clearly distinguish between emotion and sentiment. Love as a *sentiment*, because it is a system of organized emotions around an object, may nevertheless become at any time in presence of the beloved object a complex *emotion* of love[2]. An emotion is not a sentiment, but a sentiment may realize itself emotionally, that is, if my theory of emotion be accepted. According to the instinct theory of emotion, the emotions are defined as so many cut and dried states; and if so defined, it is obviously inconsistent to call the organization of them around an object also an emotion. From my account of emotion, however, the sentiment, however complex and however organized as a capacity it may be, is, when as intense feeling it is actually experienced and expressed, as much an emotion as the other emotions into which it may be analysed by instinct psychology.

To compare this view with that of Ribot is to return to our criticism of McDougall. Ribot's definition is as follows: "emotion is in the order of feeling, the equivalent of perception in the intellectual order, a complex synthetic state essentially made up of produced or arrested movements, of organic modifications...of an agreeable or painful or mixed state of consciousness peculiar to each emotion...it is always related to the preservation of the individual or the species, directly as regards primitive emotions, indirectly as regards derived emotions." With regard to the first part of this account, my criticism would be a repetition of what I have said before. Emotion or emotional feeling must be distinguished from the emotion psychosis. Emotion is not *made up* of movements, 'organic modifications,' etc. It involves them, certainly, but it is itself, among other things, the subjective feeling aspect of these processes. With regard to the second half of the definition, my criticism would be a denial. Although the emotions which man shares with the higher animals are related to the preservation of individual or species, there are, I believe, other emotions (*e.g.* the aesthetic) which cannot be said to be so related, that is if the term 'preservation' be interpreted biologically, as is clearly intended. Once again, emotions are wider than instincts.

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[1] *Op. cit.* 145.

[2] For a similar view cf. C. S. Myers, "The Nature of Sentiment,' *Psyche*, 1922, in which he distinguishes sentiment as organization from sentiment as feeling.

IV

(*a*) Having established that emotion may arise under certain conditions in any situation (and not only in instinctive situations) I may now go on to indicate the significant part which certain non-instinctive emotions I have mentioned play in the higher life of man. I refer particularly to what I have called, provisionally, 'emotions of value.' My thesis for the moment will be that at the human level, with the advent of 'reason' (as distinct from instinct or intelligence) there can arise situations in which the subject is confronted with objects (ideal or actual) which he realizes as revealing 'intrinsic,' as opposed to instrumental, value. His reaction to these objects may be emotional, and from the emotional situation may issue the very complex conations and actions which are typical of the moral, the artistically creative, and the truth-seeking life.

(*b*) Typical instances of emotions of intrinsic value[1] are those already cited, which can arise when the subject is in presence of a beautiful object or idea, or when contemplating goodness such as is realized by great moral personalities. The vivid mental realization of a moral ideal can again arouse emotion. To these one might add the emotion which the thought or pursuit of truth can stir: but this is less familiar, perhaps, and as it raises peculiar philosophical problems of its own, it is best to leave it. The typical aesthetic or moral emotions are sufficiently good examples. It does not follow, of course, that, given a beautiful or a good object, the 'emotion of value' will arise upon its apprehension. Only if the object is apprehended with intense (and complex) feeling can the experience be called emotional. But, given this emotion, it is one, I hold, peculiar to man, arising directly out of his possession of reason or the power to contemplate objectively ends or values. It is, further, not, *in se*, instinctive, however it may involve instinctive factors. And it is out of these new emotions which may thus be generated in the contact between objects revealing value and a conscious self, that there springs the dynamic energy of that characteristic part of human life which is called 'higher.' It is from this source that, *e.g.* moral 'freedom' comes, freedom which is freedom from total determination by the mere past of instinct and environment. The emotional conative reaction is still a reaction to a definite situation, but the list of such situations cannot be completed by a reference to characteristics which we share with the higher animals.

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[1] I use, as I said, the term 'intrinsic value' (*e.g.* goodness or beauty) in consonance with current philosophical usage. (Cf. *e.g.* Sorley or Alexander.) 'Instrumental' values are subservient to intrinsic values.

In the aesthetic experience, for example, we react, say, to a picture or symphony. This contemplative, 'disinterested[1],' non-utilitarian attitude may arouse intense emotional feeling which in turn issues in striving either to contemplate and absorb the beautiful object still further, or to express or create. This striving or conation, possible only at the human level, is, once again, although one may discern many instinctive elements therein, and though it is possible to show how even the aesthetic experience is vitally attached to the life of instinct[2], not primarily instinctive. But genetic psychology must be supplemented by analytic.

So, however important the existence of instincts and their emotions may be, they are not all, even if we could make a complete list of them, and they are, taken separately, as entities, or grouped together, in themselves incapable of explaining the 'higher' life. There is contained in the aesthetic experience and in all the value experiences something different in kind from instinct experiences, and it is this something different which transforms in turn the instinctive processes in man, so that we see the justice of a criticism such as G. C. Field's, that the term instinct is not competent to describe the higher life of man[3].

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[1] I use the term because it is frequently employed in this connexion, and is useful, despite certain disadvantages. Bullough's term 'distanced' (This *Journal*, v. 81–118) is useful in the case of aesthetics.

[2] See *e.g.* "The Aesthetic Categories," *Monist*, July 1921.

[3] See Art. *Mind*, July 1921.

V

(*a*) But even if this is so, it would seem that some such term as 'emergence' employed by Alexander and Lloyd Morgan, is still sufficient to account for the appearance of entirely new qualities at the human level. We have now to inquire whether this is so. If it is *not*, does this mean that there is a hiatus in evolution when we reach the human level? Or is 'emergence' an unsatisfactory conception not only at this stage, but as applied to others as well? Alexander, defining emergence, says: "The emergence of a new quality from any level of existence means that at that level there comes into being a certain constellation or collocation of the motions belonging to that level, and possessing the quality appropriate to it, and this collocation possesses a new quality distinctive of the higher complex. The quality and the constellations to which it belongs are at once new and expressible without residue in terms of the processes proper to the level from which they emerge...[1]."

The question before us is, is the 'new' emotion of value *expressible* without residue in terms of instinctive situations, or does there exist a residue, a new factor, determining the development of the higher (*e.g.* the moral) life? If emergence simply meant that new qualities 'appear' at higher levels no one would gainsay it. It means more. This being so, we have to enquire whether there is a 'residue,' and if so, what it is.

(*b*) In the first place, there is a 'residue' of innate, non-instinctive dispositions which McDougall, as we saw, admits, and until we know more of these, it is surely folly to expect to explain the life of man in terms of instinct alone. Again the term 'emergence' is misleading because you do not get a simple collocation of instincts. It is generally admitted that 'pure' instinct is a nonentity[2]. Without committing ourselves to all the implications of C. S. Myers's[3] theory, we may safely say that 'instinct-intelligence' is the safer term. When we advance beyond pure reflex to instinct, intelligence is already present, and with intelligence comes the possibility of reason and that 'something more' of which I have already spoken and to which I shall return in a moment. The higher life of man then cannot be clearly expressed psychologically in terms of instinct 'without residue,' any more than instinct can be expressed 'without residue' in terms of reflex action. There is the presence of something else.

I may further advance my thesis that there is a residue by further comparing intelligence with reason. Intelligence is present with instinct, and in man, furthermore, reason is present with intelligence. It might be sufficient to say that unless both intelligence and reason are wholly reducible to terms of instinct that they form 'residues' which make the emergence theory unworkable. But this conclusion will be reinforced if we can show what it is which *causes* a residue at all. The question, in Lloyd Morgan's phrase, "what makes emergents emerge?"[1] may give a hint, as we shall see, as to the meaning of residue in all evolution whatsoever, even although the answer to it may be beyond the scope of science and within the realms of philosophy.

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[1] See *e.g.* Alexander, *Space, Time and Deity*, II. 45–46

[2] See *e.g.* most of the papers in the 1910 Symposium (*loc. cit.*).

[3] *Ibid.*

(*c*) The clue is to be found, I believe, in what I have already said. Intelligence, we saw, discovers means to ends which in animals are undoubtedly determined by instinct. Reason goes further. It not only devises means to ends, but values these ends themselves (which mere intelligence would accept unquestioningly as final) by relating them to more ultimate ends, and finally, as is the case with moral philosophy, to some supreme end or *summum bonum* or intrinsic value. We have spoken of the emotional-conative effect which this value may have on the subject. The point here is that it is the presence of a value or end, self-consciously apprehended by the rational subject, which is the determining cause of the higher life as such, and it is *this* which produces that difference between the life of man *qua* man and of animal *qua* animal so that one cannot be expressed in terms of the other. It is the presence of end, objectively known as such, which is the cause of the irreducible 'residue' between the life of instinct or instinct-intelligence and of instinct-intelligence-reason. If I may speak of 'moral evolution,' I might add that it is the existence of the supreme value or end or purpose in human life which is, through its felt apprehension, the 'final cause' of moral evolution from the savage to the saint.

If then there is a 'residue' and it is thus caused, the theory of emergence at least on this point breaks down. No doubt as an empirical conception, 'emergence' is satisfactory enough. New qualities certainly do 'emerge' in the sense of 'appearing.' But when, *e.g.*, Alexander makes emergence an explaining principle in philosophy the case is different. You cannot eat your cake and have it. You cannot both have a new quality B appearing with a constellation of a processes, transforming them so that they are ab processes as well, and *also* say that ab processes are expressible 'without residue' in terms of a processes. You cannot, *e.g.*, have 'novelty' of mind and at the same time 'equivalence' to certain neural constellations[1]. Mere mathematical terms like 'equivalence' simply do not apply. Or to apply it to 'instinct' psychology, you cannot have a higher life of man which is in any way different in kind from animal life and yet hold it 'equivalent' to a constellation of instincts. We have shown that it is 'different in kind.' Therefore you must employ for its explanation a principle other than naturalistic.

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[1] *Consciousness and the Unconscious*, Presidential Address to the Psychology Section of the British Association, 1921. P. 3.

(*d*) It is at this point that I would like to add a very speculative hypothesis, which really carries me beyond the limits of a paper such as this. It is that the conception of end or value, which is so clearly necessary to account for the moral life, is necessary, not only to explain the development of this higher life, but to explain *all* evolution. I should like to refer to a recent paper[2] by Percy Griffith, in which the writer puts forward an hypothesis which, although familiar enough in philosophy, is yet particularly interesting because it is reached from the side of psychology. The hypothesis is that "the *universal result* of instinct activities, namely, the preservation of species...implies a pre-conceived purpose" and that "we have *instinctive activities as those characterized by foresight not personal to the organism concerned.*" Again, "*personal intelligence originates in impersonal intelligence.*" With some such hypothesis I should be in complete agreement, and I think that it might, further, be universalized. That which becomes self-consciously and personally (though imperfectly) realized at the level of reason, and which is the main cause of higher development in the individual, namely, a supreme end or purpose or value of life, is the same which 'impersonally'[3] determines the evolution from chemical to vital to mental. Only by some such conception can we explain 'residue' or 'novelty.' That, as I say, is a highly speculative hypothesis, and as metaphysics is beyond the scope of this already too lengthy paper. The point to be emphasized here is that no collocation of instinctive emotions in themselves, with the most that they imply, is sufficient to *account wholly* for the higher life of man. Objective apprehension of value is of a different kind, and when felt deeply issues in actions of a different sort from those of instinct, which, though they may be said to *appear* from a lower group of processes, are not explicable thereby. Value realized in conduct or artistic creation does not simply 'emerge.' Value in reality is the condition of all emergence.

After stating the instinctive theory of action, we questioned its validity. Then, summarizing briefly the nature of instinct we criticized

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[1] See e.g. *Space, Time and Deity*, II. 14, footnote.

[2] "The Intelligence of Instinct," *Psyche*, October 1922.

[3] I have no space to quarrel with this word of doubtful usefulness, which may quite well be taken as meaning *finitely* impersonal.

McDougall's views, and found that to regard emotion as the affect of instinct is an unproved assumption which begs the question logically and fails psychologically. We then defined emotion for ourselves as "the marked psychical affect arising in certain complex situations." Its concrete character explains its 'peculiarity.' Though emotion is feeling, it is not pleasure-pain. Feeling is a *genus* of which there are many *species*. That intensified feeling (which implies complication) may become emotional is shown by the example of a sensitive child. McDougall is right so far as his classification of instinctive emotions is a classification of instinctive situations; but exhaustiveness is impossible. Our view accounts for *e.g.* shyness and modesty as emotions: any of the sentiments may become emotions. There are also 'value' emotions which may occur at the level of 'reason,' reason involving, not merely the power to syllogize but also to contemplate and to feel intrinsic values or ends in themselves. Value emotions (*e.g.* aesthetic and moral) are not reducible to terms of instinct but are 'new.' 'Emergence' aims at expressing this in terms of its components, whilst retaining the novelty. This is self-contradictory. It is the existence of value self-consciously realized which constitutes the novelty of the moral life; reference to instincts is insufficient. So it *may* be 'impersonal' value or end which determines emergence of *all* novelty. It cannot be *vice versa*.

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A NEW TYPE OF PURSUIT-METER.

By GEORGE MACFEAT WISHART.

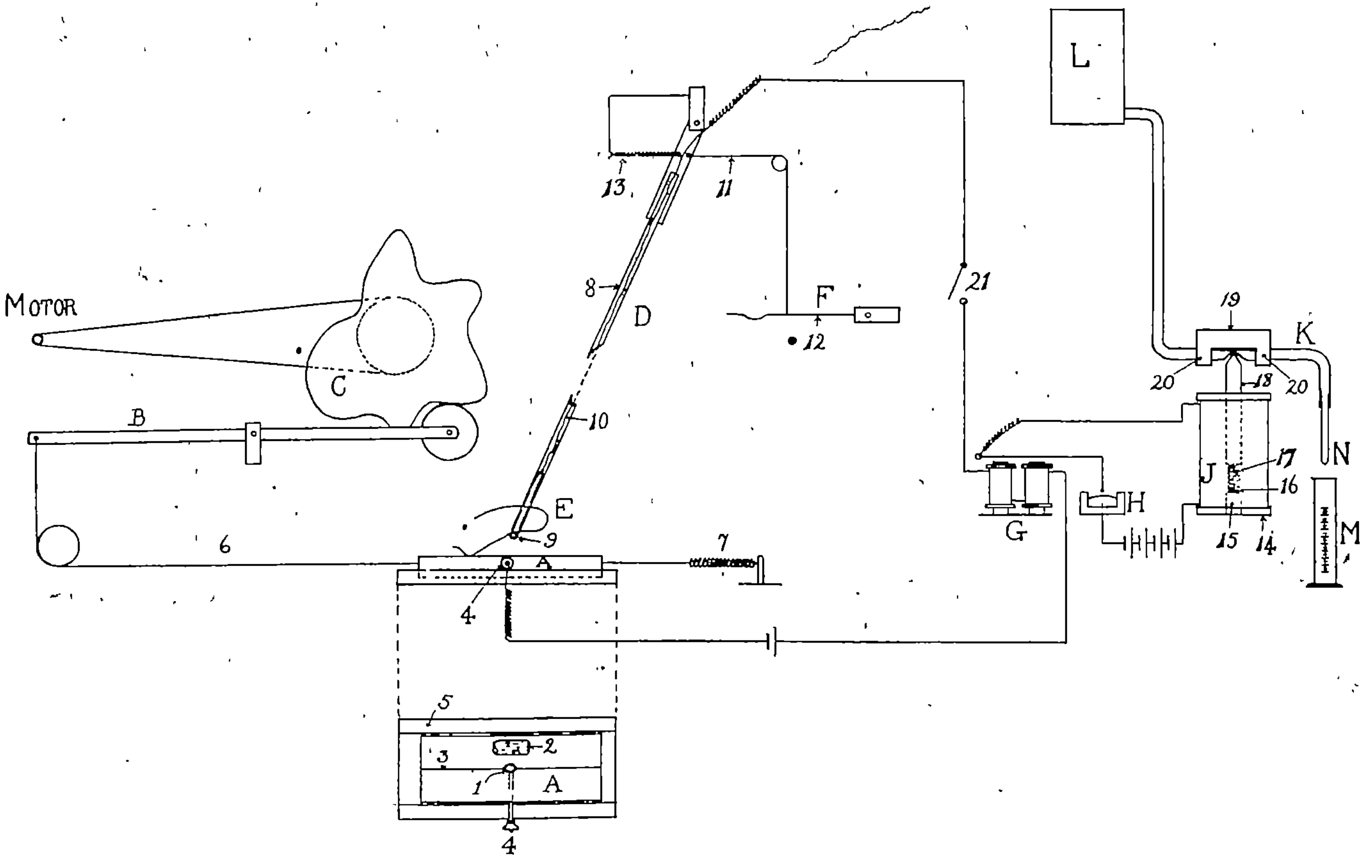
(*From the Institute of Physiology, University of Glasgow.*)

A DIFFICULTY with all types of pursuit-meters is that of eliminating the effects of practice. With the following design, the direction and the speed of the movement to be followed may be so extensively varied, that memorising the movements becomes almost impossible, and consequently the increase in proficiency with successive attempts reduced to a minimum. In addition, the apparatus would appear to offer other advantages in its adaptability to various types of body-movements, and in giving directly, without calculation, a reading which is a percentage of the maximum possible. Despite its apparent complexity, it may readily be constructed from materials to be found in most laboratories.

A general description of the apparatus as a whole will be given first, and followed by constructional details of its component parts.

The apparatus consists of a fibre-block (*A*), carrying a mercury-cup (1), this block being oscillated mechanically in a quite irregular fashion by means of the rocker (*B*), which is operated by an irregularly-cut cam (*C*). Suspended directly above the cup is a long lever (*D*), ending in a trailer (*E*), which moves in a groove cut longitudinally in the fibre-block. The lever and trailer are so pivoted that, by increasing or diminishing the pressure on the finger-lever (*F*), they may be made to follow the movements of the mercury-cup. The trailer (*E*) will float on the surface of the mercury in the cup, and, so long as it does so, an electrical circuit through the relay (*G*) is completed; the armature of the relay is held against the poles of its electromagnet, thus causing a prolongation of the armature to dip into the mercury in cup (*H*). By this contact, a second electrical circuit is completed through the electromagnet (*J*). While the current in this electromagnet is flowing, its cylindrical armature is held clear of the rubber tube (*K*), and water is allowed to flow freely from the reservoir (*L*) into graduate (*M*).

This arrangement permits of water flowing so long as the trailer is kept floating on the surface of the mercury in the moving cup, and any failure to follow accurately the movements of the cup is registered by the breaking of the circuits and the consequent cessation of the water-



Plan of Fibre block and Guide

flow. By adjusting the aperture of the nozzle (*N*), and the height of the reservoir (*L*), it is possible to provide for a flow of water amounting to 100 c.c. in a definite period of time—say two minutes. By then limiting the duration of the experiment on the subject to exactly two minutes, the amount of water collected in the graduate gives directly the percentage accuracy with which the subject has followed the movements of the cup.

Fibre-block. This consists of a rectangular block of fibre of a length sufficient to provide for the extremes of possible movement of the trailer. Midway between the two sides a longitudinal shallow groove (3) is cut, and at its central point a hole (1), 5 mm. in diameter and of sufficient depth to accommodate a large globule of mercury, is drilled. It is a convenience to cut a second mercury chamber (2) close to the first and drill a communicating passage between the two; this larger chamber can also be filled with mercury, and provides a supply to maintain the mercury in the smaller chamber at constant level despite the displacement of minute globules from its surface by the movements of the trailer. The floor of the groove must be smooth, particularly where it leads into the cup at either side; also, the level of the mercury in the cup must be so adjusted that the trailer will move easily from the groove on to the surface of the mercury without any tendency to stick at either side.

From the surface of one of the long sides of the block, a hole is drilled, penetrating into the mercury chamber; into this hole is driven a short piece of brass rod (4), with two terminal nuts screwed on to its external end; this serves as an electrical connection to the mercury cup.

The guide (5) is constructed from a piece of hard wood, very slightly broader than the breadth of the fibre-block, small slips of wood being secured along each of the longer edges to act as guides. The surface of the wood block and the projecting parts of the guides are well black-leaded.

Into each end of the fibre-block is secured a ring or hook, one serving for the attachment of the string (6) connecting it with the rocker, the other for the attachment of a spring (7), which maintains string (6) taut and keeps the pulley of the rocker constantly against the cam.

Rocker. The rocker is self-explanatory and requires no description.

Cam. Cams are conveniently cut from cardboard, and may be readily fixed to the shaft of an old recording drum by being jammed between two wood bosses, these bosses being fixed to the shaft by means of set-screws. For the purpose of driving the shaft very little power is required; the small type of electric motor used for operating a sewing-machine has been found very satisfactory.

Lever and Trailer. This was constructed from one of the levers used in physiological work for recording the movements of the frog's heart. The ordinary writing-point was replaced by a strong straw (8) about 60 cm. long. One end of this was fixed to the metal part of the heart-lever; into the other end was fixed a large pin (9), bent at a right angle so as to serve as a pivot for the trailer (*E*), which was constructed from a piece of copper wire bent to the shape shown. To the end of the pin within the straw a fine flexible wire (10) was soldered, carried up within the straw to the pivot of the lever, and brought out in a spiral to a convenient terminal.

Near the axis of the lever a string (11) is attached and carried over a pulley to the small finger-lever (12), made from another heart-lever. The spring (13) serves to keep this string taut, and pulls up the finger-lever whenever the pressure of the finger on the finger-lever is released. Such an arrangement is adapted for recording the accuracy of pursuit by finger movements, but, obviously, by suitable mechanism, the apparatus could be adapted for other types of movement.

Recording Apparatus. It has been found advisable, in place of working electromagnet (*J*) directly, to operate it through a relay (*G*). The current required to operate the armature of (*J*) against a spring sufficiently strong to compress rubber tube (*K*) is considerable, and causes sparking between trailer (*E*) and the mercury in the moving cup; this results in the mercury rapidly becoming dirty and the trailer then fails to slide easily over the surface of the mercury and occasionally fails to make contact. Interposing a relay makes it possible to use minute currents in the trailer circuit with almost entire elimination of the sparking between trailer and mercury.

Any form of relay will serve the purpose, the more sensitive the better; its armature should be light in order to render it as rapid in action as possible.

The electromagnet (*J*) was constructed by winding a wooden bobbin (14), 7 cm. in diameter and 9 cm. in length, with a core 23 mm. in diameter, with No. 20 D.C.C. copper wire. The core of the bobbin was drilled out and into one end an iron plug (15) was fixed. On the top of this plug was placed a small rubber pad (16) and a spring (17). An iron rod (18), of such a size as to be an easy sliding fit within the drilled core of the bobbin, and with its external end ground to a blunt edge, was then inserted.

To hold the rubber tube (K) in position, a small piece of wood (19) of inverted U shape was used, the legs of the U (20) being drilled to an easy fit for the rubber tubing. The base of the U has a shallow slot cut crosswise in it; this prevents the plunger (18) from turning, and ensures that each time the plunger compresses the tube, it will register across the tube and not along it.

The nozzle (N) consists of a piece of glass tubing of which the end has been almost completely sealed in the flame. Having constructed one of suitable aperture to give a flow of approximately 50 c.c. per minute with the head of water-pressure available, the flow can then be accurately adjusted to exactly this figure by altering either the height of the nozzle or the level of water in the reservoir (L).

Operation. The level of water in L is adjusted, a dry graduate placed in position, switch (21) being open. The subject is then allowed to follow the movements of the cup for half-a-minute in order to accustom himself to the pressure required to move the lever and its trailer. Switch (21) is then closed and simultaneously a stop-watch is started. At the end of exactly two minutes the switch is opened and the level of water in the graduate is read off.

(*Manuscript received* 11 *May*, 1923.)

CRITICAL NOTICE.

By T. PERCY NUNN.

The Nature of 'Intelligence' and the Principles of Cognition. By Prof. C. SPEARMAN. London: Macmillan and Co., Ltd, 1923. Pp. viii + 358. Price: 15*s*. net.

It is a familiar reproach against psychology that, judged by the standard of such disciplines as physics or even biology it is far from having attained the character of a true science. Its very data, say the critics, are vague and disputed, and as for interpretations—*quot homines tot sententiae.* In extenuation psychologists sometimes plead the youth of their study, but a subject upon which Aristotle wrote a formal treatise cannot fairly claim indulgence for its vagaries on the ground of tender years. And as a matter of fact the divergences of standpoint and method which offer so good a target for hostile comment have tended to increase rather than to diminish in recent times. Viewing the present incongruity between the schools, a bystander might well hold that James was too optimistic when he declared of psychology that although not yet a science it was the hope of one. It is true that psychologists claim credit for important achievements in education, medicine, industry and elsewhere, but these are hardly the results of the applications of a science in the sense that wireless telegraphy, for instance, is a result of the application of Maxwell's physics of the ether. They are due rather to mere recognition that in such fields of practice psychological factors have a significance much greater than was previously admitted, and are akin to what Bacon called 'first vintages' of observation over a sufficiently wide area. They may offer material for psychological theory but cannot be regarded as its indubitable offspring.

In the book before us, Prof. Spearman claims nothing less than to have removed these too valid grounds of reproach and to have supplied psychology with a body of principles comparable with the first principles of physics. It is a large claim, even though confined to the psychology of cognition, and if substantiated will secure for its author an enviable place in the history of science. Indeed, it is so large, and is supported by so masterly an array of analyses, criticisms and arguments that one must refrain from hastily pronouncing judgment upon it. Before that can safely be done the theory must be thoroughly digested and confronted in detail with the apparently conflicting views of other thinkers; above all, it must pass the crucial test of showing that it is a fruitful guide to novel investigations. Meanwhile, candour compels at least one reader to admit that, although the general framework of his ideas is very different from Prof. Spearman's and he worships gods whom the latter would overthrow, he has been profoundly impressed by a work which, whatever place it may ultimately assume, is unquestionably one of signal importance.

As the basis of his scheme Spearman lays down three 'noegenetic' principles which are, between them, generative of everything that emerges in the field of cognition. The first operates upon 'lived experience'—a term which includes sentience (*i.e.* uncognised results of sense-stimulation), affections such as pleasure, unpleasure, tension, etc. and conation—and is manifested in the tendency of all such experience "to evoke immediately a knowing of its character and experiencer." This tendency works out in processes (processes of 'apprehension of experience') which, starting from the state of ignorance, arrive at end-states having the characters of a coherent, believed, self-evident proposition, and are regarded by Spearman as playing a part in mental life comparable with cell-growth in the body. It will be noted that the principle is framed in terms which reveal the author as standing by the orthodox view that the ego is given immediately in all apprehension of experience; also that the word 'tendency' is meant to leave room for the view that lived experience may, on occasion, remain uncognised. Spearman, it may be said here, keeps severely aloof from the Freudian movement and indeed looks at it a little *de haut en bas*; nevertheless, he gives a large place to experience which lies below the limen of manifest consciousness.

According to the second noegenetic principle, the range of cognition is vastly extended by the 'eduction' (i) of relations between the characters of lived experience, and (ii) of relations of higher order between 'fundaments' which are themselves relations of lower order or complexes of cognised characters and cognised relations. A happy illustration is "the understanding of Bach's masterpieces, where contrapuntal relations have to be cognised as themselves having interrelations, and these having yet higher ones in an unending pile." The crucial distinction between the 'eduction' of the second principle and the 'apprehension' of the first is that by eduction characters come into cognition which have never been elements of lived experience—as in the conspicuous instance of mathematical relations. It is, however, admitted to be a nice point whether some relations may not, like their fundaments, be apprehended directly in lived experience, and there are not wanting metaphysicians who would challenge even the general validity of the sharp distinction between apprehension and eduction. It must be said that Spearman invites challenges of this kind; for he not only skirts pretty frequently the coasts of metaphysics—as a psychologist can hardly avoid doing—but also discloses philosophical prepossessions whose connexion with his psychology it would be at least interesting to explore. We must, however, keep closer to our task and be content to note that by the second principle Spearman is able to show very convincingly the invalidity of the old hard and fast division between perceiving and thinking. As he well remarks, a person faced with the Maze test may say with equal propriety either that he is *looking* or that he is *thinking* how to get in, while no one would rank as no more than sensory perception the process of understanding a musical masterpiece as it was described above.

The third principle introduces an idea which, though having an obvious kinship with Stout's 'relative suggestion,' is perhaps the most original and fruitful in the book. It is called "the eduction of correlates," and states that the presentation of any character together with any relation tends to evoke immediately the knowing of a correlative character. A simple instance would be the presentation of a tone between which and a given tone there is to be a given interval, for example a fifth; from this perceptual region the sway of the principle extends to the remotest provinces of abstract thought and creative imagination.

Between them the three noegenetic principles account for everything that finds its way into the cognitive field "from the loftiest flight of genius to the prattle in the nursery," but cognition involves the category of quantity as well as quality, and in this respect falls under the operation of five supplementary principles. The first asserts the conservation of mental energy—defined as the product of extensity by intensity, the next two are the opposed principles of retentivity and fatigue, the fourth refers to the conative control of the intensity of cognition, while the fifth recognises that differences in the manifestations of the other four are based upon native potencies which vary from one individual to another. From these principles are derived three anoegenetic processes which, in conjunction with the noegenetic three, suffice to explain all cognitive events. They are: reproduction, which is an expression of the principle of mental energy modified mainly by the associative principle which is included in the more general principle of retentivity but also in part by the principle of fatigue; disparition, *i.e.* the tendency of items to disappear from the cognitive field; and clearness-variation. The latter two, like the first, are products of the concurrent operation of the principles of mental energy, retentivity and fatigue, but have their distinctive forms. Disparition, for instance, accounts for the isolation of constituents within a cognitive process; while clearness-variation is obviously connected closely with attention.

In the last chapters of the book the six simple processes are applied in the analysis of the traditionally recognised cognitive functions—perception, conception, judgment, reasoning, memory and imagination. Here the reader receives his reward for mastering the necessarily abstract discussions of the earlier pages and, as the 'faculties' so long regarded as independent entities dissolve magically into common elements, rapidly becomes disposed to admit all that Prof. Spearman claims for his discoveries. In a cooler moment he may perceive that positions are sometimes taken which the author may have to defend against well-equipped opponents, but there can be no question that the method, applied with the highest skill and subtlety, simplifies, unifies, and illuminates the whole realm of cognitive psychology in a most remarkable way.

In a brief notice it has been necessary to confine attention to the main lines of Spearman's argument and to ignore secondary matters, often of high interest. Mention must, however, be made of a vigorous and interestingly documented polemic against the psychologists who identify thought with language, images, muscular sensations, etc. or regard these as symbols of which the thought itself is the mystically conceived 'meaning.' In this connexion he insists upon the existence and importance not only of 'imageless thought' but in general of a 'fluid awareness' which is to be included neither in perception nor in conception. "The cognitive field may, then, be compared to an ocean studded with icebergs. Over much the larger portion...it is still fluid. Only dotted here and there, has the thought frozen into verbo-conceptual rigidity."

A final word must be given to a point which will surprise those who know Prof. Spearman only by his formidable essays in mathematical psychology. They will be prepared to find that he appeals constantly to laboratory experiments and the results of mental tests, but may be astonished to discover that he is intellectually a son of the great medieval Schoolmen and openly acknowledges the affiliation. Yet a combination of what is essentially the scholastic outlook and method with an appreciation of the significance of the 'modern instances' which he has done so much himself to create is the most striking feature of a very remarkable and perhaps epoch-making book.

PUBLICATIONS RECENTLY RECEIVED

Economic Motives: A Study in the Psychological Foundations of Economic Theory, with some Reference to other Social Sciences. By Professor L. C. DICKINSON. Harvard University Press. 1922. Pp. vii + 304. 12*s.* 6*d.* net.

The general plan of this book is most admirable. The execution of the plan leaves much to be desired. To bring economic and psychological theories into closer relationship is a task that ought long ago to have been undertaken. What is particularly needed is an approach to psychology through economics. But Professor Dickinson, though he appears to be well equipped to make this approach, has chosen to set out from a reconsideration of ancient and current psychological doctrines. Thus two-thirds of the book present what is practically little more than a psychological text-book written mainly from a historical point of view. This part of the author's work is accurate, and interesting, but not any better than much similar work by other writers, and in no way original. The third section, entitled "Some applications of Psychology to Problems of Economic Theory" is more significant, and is a real contribution to its subject. If this had been combined with a more specialised study of the applications at work in some special community, a first-rate book might have been produced. As it is, Professor Dickinson's study was well worth making, and should point the way to further research on the application of psychology to economics.

Psychology and Politics. By W. H. R. RIVERS. With a Note by Prof. G. ELLIOT SMITH, and an Appreciation of the Author by Dr C. S. MYERS. The International Library of Psychology, Philosophy, and Scientific Method. London, 1923. Kegan Paul. Pp. vii + 181. 12*s.* 6*d.* net.

The first three of the lectures in this volume were delivered in London in connexion with Dr Rivers's parliamentary candidature in 1922. They express, therefore, some of the latest views in social psychology which Dr Rivers adopted. The first lecture is in the main a clear and simple restatement of views elsewhere put forward, but the second and third contain some remarkable fresh suggestions—and suggestions which Dr Rivers himself considered of considerable importance. Ever anxious—as Dr Myers remarks in the finely balanced Appreciation which fittingly concludes the volume—"to give a strict definition to words which had hitherto been ambiguously or loosely used," Dr Rivers was profoundly discontented with the confusion commonly prevailing between 'suggestion' and 'faith.' His distinction between the two forms of social group, that without a leader, and that with a leader, enabled him to state what he regarded as the essential difference. The leaderless group belongs to the protopathic level, and suggestion, the characteristic process of the herd instinct in that group, is an 'all-or-none' reaction. The group with a leader has arrived at the epicritic level, and faith, the characteristic process of the herd instinct at this level, is discriminative and adaptive. It is a great misfortune that Dr Rivers did not live to work out in detail the significance of this distinction. It is clear that the distinction as it stands is far too simple; for the group without a leader is not necessarily more primitive, or at the protopathic level, while leadership expresses itself in a great variety of forms; and consequently it is impossible to draw large psychological generalizations from such incompletely analysed representations of social life. Dr Rivers was fully aware of this and intended to work at the problems which this provisional analysis presented, an intention which was only frustrated by his death.

Apart from these specifically psychological lectures the most important thing in the volume is the Lecture on "The Aims of Ethnology." The arguments for culture transmission as here presented seem conclusive. The only doubt arises after a perusal of Prof. G. Elliot Smith's Note. The impression left by reading this note is that there is more heat present in the attack upon "Freud and his followers" and those whose ethnological theories they have embraced than is usual where there is scientific confidence.

Fundamental Conceptions of Psycho-Analysis. By Dr A. A. BRILL. London: George Allen and Unwin, Ltd. 1922. Pp. vii + 344. 12*s.* 6*d.* net.

The material of this book is, says the author, "taken from the lectures given at my elementary course in the department of pedagogics of the New York University." The stamp of the lecture is, in fact, to be found on every page of the book. No doubt this accounts largely for its breezy and unconventional style. It may also account for some of the dogmatism that is revealed, although this is a characteristic which probably goes more deeply into the author's personality. While the book cannot be regarded as adding anything very fundamental to the treatment of its subject, it is a lively and interesting presentation of the views of Freud. The psychologist who still manages to maintain an interest in normal mental life will find Dr Brill's book very well worth reading, if only for the fact that it will continually rub him up the wrong way. Most of the topics beloved of Freudians are discussed—forgetting, the dream, wit, slips of the tongue, common forms of insanity, only children, and fairy tales. And Dr Brill has no difficulty in solving all of the problems of all of these.

Handbook for Mental Nurses. Seventh Edition. Published under the authority of the Medico-Psychological Association. London: Baillière, Tindall and Cox. 1923. Pp. xiv + 615. 6*s.* net.

This new edition of a well-known handbook has been rewritten and considerably enlarged. The most substantial additions are exceedingly welcome, dealing as they do with the anatomy and physiology of the Nervous System and with psychology. The sections dealing with these matters are all unusually well done. They are clear and, within their limits adequate, and should attain their purpose of stimulating further interest. This is particularly the case with the section entitled *The Mind in Health*, for which Professor T. H. Pear was largely responsible. The handbook is indeed an admirable work. The book is well produced, and is remarkably inexpensive: it should more than maintain its present popularity.

The New Psychology and the Parent. By Dr H. CRICHTON MILLER. Jarrolds (London), Ltd. 1923. Pp. 255. 6*s.* net.

The sincerity and idealism of Dr Crichton Miller are manifest throughout this book. Moreover there is a large element of enlightened common sense. But it is a question whether these things are a justification for writing the book. Nearly all the topics dealt with are more suitable for scientific research than for popular and confident exposition. The education of the parent cannot be achieved by specific practical applications of incomplete and uncertain generalisations. With characteristic insight Rivers, in his last book—*Psychology and Politics*—uttered this warning: "There is now a serious danger that psychology will fall into discredit" mainly "owing to premature attempts to utilise its supposed discoveries practically while the basis upon which they rest is uncertain and insecure." Dr Miller's book illustrates the danger.

Miracles and the New Psychology. By E. R. MICKLEM. Oxford University Press. 1922. Pp. 143. 7*s.* 6*d.* net.

After a short general introduction, Mr Micklem deals briefly with some of the outstanding characteristics of modern psychotherapy. He then discusses beliefs current in New Testament times with regard to the relation between sin and suffering, and the influence of demons. This leads him to a detailed examination of the cases of miraculous cure reported in the New Testament. His general conclusion is that "we cannot show with certainty that any case in the N.T. has its parallel in the annals of modern healing by psychotherapy. The reason for this conclusion is that sufficiently definite indications are not given as to the nature of the disease."

The essay possesses considerable interest and is well written. The author does not appear, however, to have had much first-hand experience of modern psychotherapy.

Expressionism in Art. By OSKAR PFISTER. Authorised translation by BARBARA LOW, and M. A. MÜGGE. London: Kegan Paul, Trench, Trübner & Co., Ltd. 1922. Pp. vii + 272. 6*s.* 6*d.* net.

The first 141 pages of this book are occupied by an account of a "fragmentary analysis" of a certain "expressionist" artist in relation to some of his drawings. Having then pointed out that this analysis is wholly insufficient to yield any definite results, the author remarks that "certain art-psychological conclusions can be derived from our fragment." These conclusions occupy the remainder of the book. They add practically nothing, beyond a number of ponderous terms, to what has often been said already. The translation has no charm and little form, and no small part of the writing appears almost as incoherent as the art which it professes to analyse.

Reports of the Industrial Fatigue Research Board. Nos. 19–23:

No. 19. *Two Contributions to the Study of Accident Causation.* Pp. xi + 36. 1*s.* 6*d.* net.

No. 20. *A Study of Efficiency in Fine Linen Weaving.* Pp. iii + 28. 1*s.* 6*d.* net.

No. 21. *Atmospheric Conditions in Cotton Weaving.* Pp. x + 36. 2*s.* net.

No. 22. *Some Studies in the Laundry Trade.* Pp. v + 57. 2*s.* 6*d.* net.

No. 23. *Variations in Efficiency in Cotton Weaving.* Pp. vi + 60. 3*s.* net. All published by H.M. Stationery Office.

Third Annual Report. Pp. 83. 2*s.* net.

These six publications worthily uphold the high standard of the reports published by the Industrial Fatigue Research Board, and represent a great amount of work well planned and thoroughly executed. While Nos. 20, by H. C. Weston, and 21 and 23 by Stanley Wyatt are perhaps of chief interest to the textile operative or employer. No. 19, by Ethel E. Osborne and H. M. Vernon, and Bernard Muscio, and No. 22 by May Smith have a very general psychological value. Muscio's investigations point to the conclusion that "the principal factor in the hourly variations in the number of industrial accidents is *not* 'fatigue,' but *rate of work.*" His call for further experiments however is undoubtedly well justified. Miss May Smith's studies of laundry workers are admirably discussed. She deals with variations of efficiency, their individual differences, their relation to physiological disturbances, their susceptibility to social influences, the effect upon them of rest pauses, and atmospheric conditions. Any student in search of subjects for psychological research might find many suggestions in these reports. The *Third Annual Report* summarises the work of fifteen months, and includes a number of extremely interesting "personal contributions from investigators."

The Measurement of Emotion. By W. WHATELY SMITH. London: Kegan Paul, Trench, Trübner & Co., Ltd. 1922. Pp. 184. 10*s.* 6*d.* net.

This work is for the most part a reprint of articles which have appeared in the General and Medical Sections of this *Journal.*

The Emotion—or, as the author would prefer to call it, 'affective tone'—produced by word association tests was measured by three methods, viz. the psycho-galvanic reflex, the reaction time, and the reproduction test.

From the effect of affective tone on memory Mr Whateley Smith deduces the existence of two opposite kinds of affective tone, which he calls positive and negative. The substitution of this classification for the more common one of Pleasure-Pain, and the Reality Principle, is well defended in the last chapter. It is shewn that the reproduction test is the most reliable form of complex indicator; and suggestions are made for the application of the association test with the psycho-galvanic reflex to measure individuality and for use in the investigation of cases of dual personality.

Jung's classification of associations is improved upon; while experiments on the effect of alcohol are discussed, and the possibility of the use of the psycho-galvanic reflex in the study of the psychological action of drugs is suggested.

One closes the book with a feeling of discontent. One feels that the manipulation of the data is perhaps too dexterous; that they are being made to yield more than they *legitimately* can; and that the author may, unwittingly, be led at times to certain manipulations which best fit in with his theories. Still the work is of interest, and will be of help to others engaged in the study of 'emotion.'

Human Character. By Hugh Elliot. London: Longmans, Green & Co. 1922. Pp. xvi + 272. 7*s*. 6*d*. net.

Reason is treated as the handmaid of the emotions—variously called 'passions,' 'instincts,' and 'feelings'—which are the directors and controllers of human action. The three primary emotions are egoism, love, and social and moral feeling. From these three the other 'passions,' *e.g.* jealousy, religion, friendship, greed, vanity, custom, etc., are derived. They are discussed in the first eight chapters. Thinking and action as methods of relieving emotion are then studied, and further chapters are devoted to belief, fanaticism, genius, vice and crime, the effects of disease, heredity and environment.

The work is a readable presentation of the subject. One feels that its value, even to the 'average reader,' would have been increased by a little less looseness of expression, and if the author possessed a more intimate knowledge of the developments of modern psychology, which is scarcely the 'stronghold of metaphysics' he supposes.

Behaviourism and Psychology. By Dr A. A. Roback, Harvard University, University Booklore, Inc., Cambridge, U.S.A. 1923. Pp. 284.

The book first traces the growth of the behaviouristic school and attempts to display the interrelations of some of its exponents. Next follows a critical examination of its bases and teachings. This analysis reveals what the author considers to be the fallacies and weakness in the theories of 'behaviourism.' The value of its principles is further tested in reference to the mental life, ethics, jurisprudence, medicine, religion, and social conduct, and is again found wanting. The present influence of behaviourism is attributed chiefly to propaganda. The last chapter discusses the future Psychology, and maintains that greater attention must be paid to the affective side, as distinct from the cognitive, to the experimental laboratory, and to the psychopathic wards.

This book makes a strong case against 'behaviourism.'

The Founder of Quakerism: A Psychological Study of the Mysticism of George Fox. By Dr Rachel Knight. London: The Swarthmore Press, Ltd. 1922. Pp. 280. 12*s*. 6*d*. net.

In its promise this book is of first-rate importance. Psychological biographies, not unduly biassed in any one direction, could contribute much valuable data for the understanding of human thought, feeling and action. But in its performance this particular book is most disappointing. The comparisons—with a large number of circular diagrams—between Fox and other mystics are tiresome, and where the author does become more definitely psychological, as in her remarks on Fox's hypersensitivity, she shows an unfortunate lack of critical capacity. Nevertheless, as furnishing data for further interpretation, this biography is welcome and will be found useful. Particularly the long chapter on "conflicting traits" contains numbers of interesting suggestions which perhaps some more perfectly equipped investigator may follow up. It is only just to remark that the author died before she had thoroughly completed her study. As indicating what might be done this book deserves to be read widely by students of psychology.

The Investigation of Mind in Animals. By E. M. SMITH. Cambridge: at the University Press. 1923. Second Edition. Pp. 195. 6*s.* net.

The second edition of this little book contains only one important change. A brief account is now given of the Multiple Choice method of Yerkes, and of the experiments made by van Hamilton for the study of perseverance reactions. This replaces the former description of the 'talking horses,' and makes the book distinctly more valuable.

Observations et Expériences de Psychologie Scolaire suivant le programme des Écoles Normales du 18 *Août* 1920. Par MARCEL FOUCAULT. Paris: Les Presses universitaires de France. 1923. Pp. viii + 161. 5 fr.

This book consists of lectures of a somewhat elementary character delivered by the author on various topics connected with the experimental investigation of the physical and mental abilities and characteristics of school-children. A number of experiments are also described and their results set forth. The book is clearly written and interesting. It covers a considerable range of subjects, from anthropometric measurements, through studies of the memory, dreams, imagination, judgment and reasoning of children, to remarks on temperament and character.

Medizinische Psychologie: ein Leitfaden für Studium und Praxis. Von Dr ERNST KRETSCHMER. Zweite, verbesserte Auflage. Leipzig: George Thieme. 1922. Pp. vi + 306. Ausland: Geheftet, 6*s.* 6*d.*; Kartoniert, 8*s.* 3*d.*

This is a curious book, and it is hard to pass any very definite judgment upon it. Much of it is extremely interesting, much is merely amusing, comparatively little can be said to be original. The author deals with the general development of the psychic life, attempting to show the significance for modern psychology of the study of primitive art and other departments of early social life. He discusses some of the principal human impulses or tendencies, and their expression in the temperament. And he deals with personality in regard to certain typical modes of behaviour. A final section discusses methods of practical psychotherapy. Whatever may be thought as to the adequacy of the foundation for Dr Kretschmer's views, he has undoubtedly a gift for discovering interesting illustrations.

Über Hysterie. Von Dr ERNST KRETSCHMER. Leipzig: George Thieme. 1923. Pp. vii + 115. Ausland: Kartoniert, 2*s.* 4*d.*

In this short treatise Dr Kretschmer discusses first the relation between hysteria, instinct and individual bias (*Trieb*), and secondly the psychophysical mechanism of the motivation of hysteria. The treatment is interesting though not very distinguished.

Experimentelle Untersuchungen über das Gefühlsleben des Kindes im Vergleich mit dem des Erwachsenen. Von HELGA ENG. Kristiania.

The writer sets out by means of extensive experiments upon children and adults to prove what differences, if any, exist between the affective life of children and that of adults. The apparatus used was the plethysmograph in conjunction with a pneumograph and kymograph; the subjects were school-children, boys and girls between 10–12, and young University students, men and women.

The problems to be investigated were as follows:

(1) Whether psychical events of a certain type are accompanied by a certain change in pulse volume and respiratory curve.

(2) Whether the vasomotor and respiratory changes of pulse and respiration to be found in children possess any characteristics peculiar to the child, if so, what this may be and what it signifies.

Among the first tests were those investigating changes found during a state of attention and tension, and the correlation between the two, the conclusion arrived at being that tension shows the same expression as voluntary and involuntary attention.

A series followed upon attention applied to intellectual work. Experiments were afterwards made with taste and smell stimuli causing unpleasure, given to both children and adults, with pain and shock stimuli causing unpleasure (omitted in the children), and with taste and smell stimuli causing pleasure, to children and adults.

It was found that great changes were made in the graphs by spontaneous affect, pleasure and unpleasure, arising from within the subject during the experiment, often quite unconnected with it. Spontaneous unpleasure was more seldom found than pleasure, in children; with the adults it was more common. Psychological unpleasure was often caused by weariness at sitting still and expressed itself first in depression of pulse and respiration, with characteristic rise at the end of the curve. Psychological pleasure had but little effect upon the vasomotor system. This was thought due possibly to the difficulty, especially in adults, of applying a sufficiently strong stimulus. Intellectual emotions were also tested. Here it was found that among the children speculation about the apparatus caused much fluctuation, especially when a lively imagination was brought to bear upon it. Boredom was immediately evident, the graph being incapable of dissimulation. In the case of adults it was found that when their thoughts were turned towards studies an immediate depression manifested itself. Experiments were made upon the aesthetic emotions with stimuli of colour (very unsuccessful), sound (rather unsuccessful), pictures, especially with the children, and reading.

No difference was found between the affective life of children and adults, the same changes in expression of affect being as regularly found in both. The spontaneous curves, however, showed great variation. In adults a curve showing spontaneous unpleasure, was found more frequently, and was of longer duration than with the children; in adults it arose from personal specific events; in children it was caused by a sensory stimulus and was wiped out by any insignificant stimulus following it. A similar difference was discernible with the curves showing spontaneous pleasure. This was more easily aroused in children than adults.

Difference in affective life between men and women.—In tests where stimuli were employed no difference was found between boys and girls, men and women; but in the spontaneous curves, much was evident. Depression was more frequent in women, unpleasure and irritability in men. The men showed more curves of spontaneous unpleasure and fewer of pleasure; with women this was reversed. The spontaneous curves of the children showed no such difference between boys and girls.

In conclusion, Frau Dr Eng correlates her experiences with those of previous investigators of the affective life. In respect to the theory of James and Lange that the organic changes regulate the affect, she says: "I should like to remark that all my observations show that the affect is primary and the vasomotor and respiratory charges secondary. A boy hears a piece of music and does not like it, the curve is unchanged; he listens to a song that he finds beautiful and the curve responds accordingly. Mosso comes to the conclusion that the Psyche under all circumstances appears to be the servant of Matter; on my part, I find the strongest reasons to reverse this thesis and to say that there is nothing which Matter must serve so much as Mind."

NOTES ON RECENT PERIODICALS

Zeitschrift für Psychologie.

Bd. 87. Hefte 3 & 4. 1921.

Über den inneren Farbensinn der Jugendlichen und seine Beziehung zu den allgemeinen Fragen des Lichtsinns. (Bernhard Herwig.)

This paper issues from the Psychological Institute of the University of Marburg and constitutes part 5 of a series entitled "Über Grundfragen der Farbenpsychologie. Zugleich ein Beitrag zur Theorie der Erfahrung," edited by Prof. E. R. Jaensch.

Previous investigations of the institute on colour-vision had been performed on adults; with the present one it was intended to supplement the method by tracing the development of colour-vision and the experiments were conducted with boys, 10–17 years of age, *i.e.* before, and to the beginning of, puberty. The observations concerned essentially the "Anschauungsbild" (intuitional image), a name given by Urbantschitsch to the phenomenon described by Fechner [G. T. Fechner, *Elemente der Psychophysik*, 3te Aufl, Leipzig, 1907, II. 484] as the "Gedächtnisnachbild" (Memory after-image), and in English psychological literature better termed the "Primary Memory Image." This, it appears, has in children almost sensory vividness, which, however, generally diminishes and eventually disappears with the approach of puberty. It must, however, not be confounded with the after-image, positive or negative, especially as, it is claimed, the "Anschauungsbild" is with some observers of a negative character, *i.e.* appears in complementary colours.

The principal results may be summed up thus:

(1) The primary memory image is a subjective visual phenomenon which occurs frequently in juvenile age (about 37 per cent.), but later declines.

(2) For most observers a background of a definite grey is the most favourable on which to project the primary memory image. Many see it best with closed eyes.

(3) After-image—Primary memory image—Memory image, represent successive stages of phenomena.

(4) The primary memory image is a central process.

(5) The primary memory image is amenable to the experimental methods of sensational and perceptual psychology.

(6) The primary memory image exhibits the properties of ordinary vision in an enhanced manner.

(7) The primary memory image is not an ordinary positive or negative after-image.

(8) The primary memory image may be positive or negative.

(9) The primary memory image may appear in feebler colours.

(10) and (11). It appeared probable that this was owing to induction induced by the surrounding grey background.

(12) The smaller the stimulus-field the greater the influence of induction in the primary memory-image.

Über Kontrast im optischen Anschauungsbild. (E. R. Jaensch.)

This is the sixth part of the above mentioned series.

Experiments with an adult observer are described which confirm the observations on simultaneous and successive colour-contrast in the primary memory image given in the preceding paper.

Über Mischung von objectiv dargebotenen Farben mit Farben des Anschauungsbildes. (Bernhard Herwig und E. R. Jaensch.)

This is the seventh part of the same series. It is claimed that these experiments furnish an objective control of the statements of the observer. The primary memory image of a coloured square is projected upon another square of the same size but of a different colour. For this mixture, or addition, of colours the same rules obtain as for the ordinary mixture of colours, viz.: (1) complementary colours mixed in definite proportion give grey, or if the proportions are different they give the predominating colour in lower saturation. (2) Non-complementary colours form a colour that lies between them on Hering's colour circle. These facts being unknown to the youthful observers furnished the objective control of their statements.

Zeitschrift für Psychologie.

Bd. 87. Hefte 5 & 6. 1921.

Die psychische Leistungsfähigkeit bei wechselnder Disposition. (Antonin Prandtl.)

This is an investigation on the influence of bodily disposition on mental efficiency. The experiments were performed on two adult males during four successive days of a week and repeated for four weeks. On the first, second, and fourth days of each week the subjects were tested in their usual state of good health whilst on the third day sickness was produced artificially by continued rotation. Five kinds of tests were applied, namely: (1) the learning of nonsense-syllables; (2) the apprehension of figures formed of straight lines shown in the tachistoscope; (3) the immediate retention of visual signs, *i.e.* of series of pairs of consonants exposed in Ranschburg's memory apparatus; (4) the learning of pairs of consonants examined by the method of hits accompanied by statements as to the certainty or uncertainty of the answer; (5) forming and writing down of nonsense-syllables.

There seems, on the whole, to be some impairment of the mental efficiency in all the tests, owing to the temporary, artificially produced state of sickness.

Experimentelle Beitrage zur Lehre vom Wiedererkennen. (Hans H. Keller.)

The experiments were performed in the Psychological Institute of the University of Göttingen on 20 observers, all of whom were university teachers or students, most of them psychologists.

The investigation started with the question: How does, under equal conditions, sensible and senseless learning-matter behave with respect to recognition? It was found that under the given experimental conditions, *i.e.* after a lapse of ten minutes, the senseless syllables were decidedly better recognised.

The next step in the investigation was a juxtaposition of longer and shorter intervals between learning and examination, the other experimental conditions remaining constant. Here it was found that with the simultaneous learning of sensible and senseless syllables after a lapse of ten minutes the senseless syllables were again generally better recognised than the sensible syllables; but after a lapse of twenty-four hours a distinct preponderance in the recognition of sensible syllables was found. The author attributes the first result to a greater amount of attention being paid during the learning to the unfamiliar senseless material and the second result, *i.e.* the quicker waning to the want of familiarity with it. The next step was therefore directed to the elimination of this disturbing influence of attention. This was accomplished by making the subjects sufficiently familiar with a certain amount of the senseless material employed. It was found that the familiar syllables had not only after an interval of twenty-four hours but also after that of ten minutes a decided advantage with respect to recognition.

The next point of the investigation was concerned with the recognition of syllables of different ages of acquaintance, and with the significance of the measures of judgment for recognition. It was found that, under the given experimental conditions, an equal familiarity with syllables of different ages of acquaintance declined during a period of twenty-four hours more slowly with syllables of longer (older) acquaintance than with those of shorter (newer) acquaintance.

The last result of this interesting series of investigations is the establishment of similar behaviour in reference to recognition to that expressed in reference to memory by Jost's first rule: Re-learning has with reference to subsequent recognition a greater value for syllables of longer (older) acquaintance than for those of shorter (newer) acquaintance.

Über formale Wahltendenzen. (Imre Hermann.)

This investigation was suggested by Marbe's paper, "Über das Gedankenlesen und die Gleichformigkeit des psychischen Geschehens," and it appears that things are not quite so simple as was made out by Marbe, for by certain subjects and under certain experimental conditions different results were obtained.

These two double numbers give also many reviews of psychological publications and periodicals. At the end of the volume there is to be found the announcement that the *Psychologische Gesellschaft zu Berlin* intends to submit to an exact scientific investigation some of the so-called 'occult facts.' For this purpose a committee has been formed to which belong for the present Dr R. Baerwald, Prof. Dr Dessoir and Geh. Sanitätsrat Dr Albert Moll. The chief problems for the work of the committee are the following questions:

(1) Is there a (spatial or temporal) second sight?
(2) Is there such a thing as telepathy?, etc.
(3) Is there such a thing as telekinesy?, etc.
(4) Are there materialisations of defunct persons or other materialisations, *e.g.* parts of the body, etc., etc.?

Any one who is convinced that any of these questions can be answered in the affirmative and that he can furnish to the committee an unconditionally convincing experimental proof is asked to communicate with Geh. San.-Rat. Dr Albert Moll, Berlin, W. 15. Kurfürstendamm 45.

Zeitschrift für Psychologie.

Bd. 88. Hefte 1 & 2. 1922.

Vocal und Ton. (Hans Lachmund.)

This is Part 2 of the series "Grundfragen der Akustik und Tonpsychologie" (Fundamentals of Acoustics and Tone Psychology) edited by Prof. E. R. Jaensch of Marburg, and forms a continuation of the first part by Jaensch himself: "Über die Natur der menschlichen Sprachlaute." It had been shown there that a vowel perception occurs when the vibration-frequency of the sound waves impinging upon the ear keeps close to a given average value. The vibration must, however, differ from a regular periodic one by the presence of a 'disturbance-factor,' that is a factor that deprives the sound-stimulus of its regular periodic character. This is accomplished by means of a selenium-siren. The stimulus-producing telephone and a selenium-bridge are introduced in the same circuit and the light-pencil that illuminates the selenium-bridge is interrupted by a rotating siren-disc. The telephone is therefore made to vibrate in unison with the wave-form imparted to the siren-disc-periphery. The quality of the vowel depends upon the average vibration-frequency of the disturbed sine-curve. If this is in the neighbourhood of 500 an O (as in *open*) is heard, near 1000 an A (as in *mar*), and near 2000 an E (as in *feign*), etc.

The chief results of the paper are summarised thus:

(1) A sound-wave whose single wave-lengths are near some mean value is heard as a vowel when its pitch is no longer perceived. To eliminate the pitch perception a "disturbance factor" (*e.g.*, wave-length-variation, amplitude-variation) in the regular sine-course of the stimulus is under normal conditions necessary.

(2) The quality of the vowel depends upon the mean vibration-frequency of this "formant-vibration."

(3) If a periodicity (the frequency of which is naturally smaller than that of the formant-vibration) should also be present in the sound-wave it can become the "voice-tone" and therefore the carrier of the vowel-quality.

Über die Abhängigkeit der scheinbaren Schallquelle; ein Analogon zu den sog. zentralen Faktoren des Farbensehens. (Hans Lachmund.)

This is a third contribution to the "Grundfragen der Akustik und Tonpsychologie" edited by E. R. Jaensch.

The experiments were similar to those described by Myers and Wilson[1]. As the author cannot conceive of a diminution of the intensity of sound with the square of the distance in the 30 cm. long metal tube, in which only a negligible fraction of the energy would be dissipated, he comes to the conclusion that the difference of the auditory impressions was not determined objectively by a difference of the intensities of the stimuli, but that it was subjective, and sees in this supposed fact an analogy to the central factors of colour-vision. The author is evidently unacquainted with the conclusion put forward in the above-named work by Myers and Wilson.

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[1] C. S. Myers and A. H. Wilson, "The Influence of Binaural Phase Differences on the Localisation of Sounds." This *Journal*, 1908, II. 363 sq.

Über die Herausdifferenzierung der Wahrnehmungs- und Vorstellungswelt aus der originären eidetischen Einheit. (Paul Krellenberg.)

This is the fourth contribution to the series "Über die Vorstellungswelt der Jugendlichen und den Aufbau des intellectuellen Lebens."

The author claims to have verified by his investigation his working hypothesis that the 'Anschauungsbild,' the primary memory image, represents the original unity out of which the perceptional and the ideational worlds differentiate themselves. His experiments were conducted with children, but no precautions seem to have been taken to guard against suggestions which is *the* great difficulty in juvenile introspection. An excellent test for suggestion, one would think, would be the projection of the visual after-image upon screens at different distances from the observer; the farther the projection the larger the after-image. Such an experiment was indeed actually performed, though not with the object here suggested, and the fact that the after-image did not increase in the same ratio as the distance of the plane upon which it was projected is relied upon as a support of his hypothesis. Of the fourteen boys whose results are given in Table I, p. 63, only one seems to be unaffected by suggestion.

Die Milchstrasse als Gegenstand der Sinneswahrnehmung. Pp. 10. (J. Plassmann.)

The author wishes to instigate psychologists to the study of the subject.

Tierpsychologische Untersuchungen. (*Versuche an Hühnern.*) (Géza Révész.)

The following questions were approached: (1) Determination of simultaneous colour-contrast. (2) Comprehension of quantity and tendency to choice. (3) Accuracy of pecking. (4) Practice effect.

The author finds that simultaneous colour-contrast exists in fowls and concludes that this fact subverts any psychological contrast theory for men. Fowls comprehend quantity, and of two heaps of grains choose the larger. Fragments of grains down to the size of 0·5 mm. were picked up with absolute accuracy. The author also establishes formal practice effect.

Zeitschrift für Psychologie.

Bd. 88. Hefte 3–5. 1922.

Untersuchungen über die Sicherheit der Aussage. (Georg Ries.)

Das Erkenntnisurteil. (F. Schumann.)

These researches are essentially concerned with the subjective experience, the 'feeling of certainty' in recognition. The experiments consisted of four groups. In the first group were given numbers and letters and in the second simple geometrical forms in different colours and simple perspective drawings of small common objects in every day use and pictorial representations of easily comprehensible actions. The third group comprised single sensible words or two given successively, some of these being mutilated. The last and fourth group consisted of smells. The investigation confirmed the results obtained by G. E. Müller as given in the 10th chapter of his *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufs.* In the experiment with smells it was found that the coëxistence in consciousness of the sensation and of the idea of the name did not suffice to bring about the judgment. Something else had to be present. In some cases it was found that this something was an explicit memory-comparison.

In the Appendix Prof. Schumann passes in review the series of very interesting and important investigations on the 'Recognition-judgment' that have been made in his institute at Frankfurt a/M., and discusses among others the work of Erdmann and Dodge, Künzler and J. Wagner, Wiegand, Ries, and H. Henning.

Beiträge zur Sozialpsychologie des Haushuhns. Mit Anhang von. (T. Schjelderup-Ebbe.)

Tierpsychologie und Soziologie des Menschen. (David Katz.)

This is a careful and interesting account of lengthy systematic observations made upon domestic hens in the experimental poultry yard of the University of Rostock. It appears that social positions are fought for by hens and once established by combat generally continue. In the appendix Katz makes very interesting and suggestive comparisons between fowl sociology and human sociology.

Über die Erblichkeit der musikalischen Begabung. Nebst allgemeinen methodologischen Bemerkungen über die psychische Vererbung. (V. Haecker und Th. Ziehen.)

This is the first portion of an investigation to ascertain the heredity conditions, the development, the components, and the correlative relations of musical ability by means of statistical material obtained by the questionnaire method. The results will be reviewed with the concluding portion of this investigation.

Psychologische Erfahrungen mit einem Zeitsignal. (J. Plassmann.)

The author describes observations by the eye and ear-method of the time-signal given at 11 a.m. by the wireless station on the Eiffel tower and records the deviations of his readings.

Physikalische Betrachtungen zum Problem der Wirkung psychischer Faktoren auf materielle Systeme. (Karl Sapper.)

The question discussed is whether, and how, a physical factor may influence a material system.

Über traditionellen Speisenabschen. Ein Beitrag zur genetischen Gefühlspsychologie. (Julian Hirsch.)

This is an exceedingly interesting paper. Hirsch distinguishes two forms of food-aversion: (1) the individual or idiosyncratic aversion, and (2) the collective aversion. The former which he does not think to be teleologically interpretable is not discussed. Of the collective aversion he distinguishes three kinds (*a*) that resulting from satiation, (*b*) that resulting from habituation, and (*c*) the traditional aversion. It is this last kind which is examined in detail. He comes to the following conclusions. A teleological foundation for traditional food-aversion is impossible. The avoidance of certain foods is not due to aversion, but the aversion is due to the avoidance. The numerous reasons given for the avoidance of such foods are merely subsequent rationalisations of traditional sentiments. These food-avoidances are of magical origin and have been maintained by the power of the instinct of imitation. The transition from avoidance to aversion has its psychological explanation in the close connexion between reverence and aversion.

L'année psychologique.

XXII année. 1920–1921.

In the preface, the Editor explains the fact that the Journal covers the work of two years, and speaks of the period as showing remarkable fecundity. In the bibliography, various researches stimulated by the war are considered, such as the problems of camouflage, gun-laying by sound, aviation, selection for military purposes, and psychotherapy. Reference is made to the development of psycho-analysis, as being mainly evident in Anglo-Saxon countries, and as adding little to the psychotherapeutic knowledge of the French.

Among the original articles are to be found:

1. *Les sensations visuelles élémentaires autour de la tache aveugle.* (M. Foucault.)

The author investigates and verifies the intermittence of colour sensations in indirect vision. He then proceeds to compare and contrast brief stimulations of the edge of the fovea and prolonged stimulations of a sensitive spot with various colours. The next series of observations were taken upon prolonged stimulation of sensitive and symmetrical spots, using a yellow paper, in comparison with the stimulation of the fovea with the same yellow paper, the subject still being the author. He then compares the perception of the sensitive spots with those of the lateral regions of the retina in regard to prolonged stimulation. In conclusion, the author assumes no new discovery as the result of his investigation, but he states that it is by no means out of harmony with the colour scheme of Hering.

2. *Recherches expérimentales sur le comportement de diverses araignées.* (Et. Rabaud.)

M. Rabaud's first theme is the spider's instinct to paralyse its prey and the order it pursues in attaining and ingesting food. Part 2 deals more fully with the capture of the prey, the methods that are used, including the use of the web and the thread thrown out from the spinnerets for enveloping the fly and the rôle played by the mechanical vibrations. The author seems to be inclined to believe in a considerable amount of independence of the different reflexes, and an explanation of the guiding line occupies a section to itself. M. Rabaud is a supporter of the theory that instinct is a matter of the reflexes, and he asserts that his subject, upon which these experiments were made, demonstrates his theory and proves its accuracy, as the spider may be relied upon always to react in a stereotyped way to a given stimulus.

3. *Nouvelles recherches sur l'analyse du temps de latence sensorielle et sur la loi qui relie ce temps a l'intensité d'excitation.* (H. Piéron.)

Here we have a detailed account of a most painstaking series of experiments attempting to arrive at some conclusion concerning the duration of sensorial latency. The first experiments were those upon the sense of taste with stimulation of sweet, salt, acid and bitter substances, the curves of the two former being closely analogous, while the latter were widely divergent.

The auditory tests were made with the stimulus of the Toulouse acoustic aesthesimeter, not a very perfect method as it is open to considerable error, but the writer was able to draw some general conclusions from his results. The next section deals with cutaneous sensations with electrical stimulation.

M. Piéron proceeds to examine the reflexes set up by electrical stimulation of the labyrinth; and the light sensations caused by prolonged excitation but without precise fixation, in states of retinal adaptation. Following upon this experiment is one upon light sensations during foveal stimulation, short or prolonged, in the region of both light and dark adaptation.

An appendix gives the notes of results of recent research upon chemical stimulation of the decerebrated frog and the thermalgic stimulation of a human subject.

4. *Les reactions motrices dans les crises dues à l'émotion.* (Henri Wallon.)

This writer gives an account of motor reactions due to emotional disturbance in a number of cases which have come under his observation, both of persons suffering from epilepsy and other major or minor troubles.

5. *A la recherche d'une sensation tactile pure.* (J. Philippe.)

An investigation carried out upon the lines of the Weber technique, but with an improvement in method, which the writer thinks leads to a smaller amount of error in his results.

6. *Recherches sur les fonctions mentalles de l'enfant à l'âge scolaire des services que peuvent rendre les examens psychologiques pour la connaissance d'une classe.* (Jadwiga Abramson.)

The first part comprises facts and introductory observations, together with notes upon collective and individual tests and the grouping of subjects. The second part contains an attempt at interpretation by the correlation method; the range of each test, and the hierarchy of the mental functions are discussed. In her summary Mlle. Abramson shows how these tests may be of service in pedagogy as well as in the study of psychology both individual and general.

Journal of Experimental Psychology. Vol. v.

No. 6, December 1922.

Personality Studies of Three-Year-Olds. (Helen T. Woolley.)

Two detailed studies of little children are given, and are valuable additions to our scanty knowledge of the social reactions of children of this age. The first, a case of a healthy and intelligent child who suddenly developed a phobia, which was successfully analyzed, is particularly striking and instructive.

The Color Preferences of Five Hundred and Fifty-nine Full-blood Indians. (Thomas R. Garth.)

There is for full-blood Indians a rather definite sequence of colour preference, the order being red, blue, violet, green, orange, yellow and white; the preference for yellow over white, however, not being very determinate. Full-blood Indians are in general aware of more decided preferences than are whites; and their preferences appear to be stubbornly native. There is a definite tendency for a preference sequence for the same seven colours in 'white' subjects, the order being blue, green, red, violet, orange, yellow and white. It would thus appear that there are clear racial differences with regard to colour preferences.

A Simple Voice Key. (F. L. Wells and J. S. Rooney.)

A discussion of the merits and de-merits of the various types of voice-keys; with a description of a simple key which is adapted from a toy called the 'Radio Rex,' marketed by the John Hugo Manufacturing Company of New Haven, Conn., at the price of $2.00.

Results of Variations in Length of Memorized Material. (Edward S. Robinson and William T. Heron.)

The memory curves for all the lengths of material studied show a general negative acceleration, and vary regularly according to the lengths of material represented. The amounts of any list learned after certain periods of study may be considered as percentages of the total material in that list learned in certain percentages of the total time required to learn that list. The number of repetitions required for complete learning increases rapidly with early increases in the length of material and more slowly with later ones; there is not only a continuous increase in repetitions with increasing length, but also a continuous increase in *repetitions per syllable.* The procedure of considering the relationship between length of material and repetitions required to learn it is not very satisfactory for determining the relationship between length and difficulty, because the *repetition* is a unit of effort which itself varies with the length of the lists employed. Using a constant unit of effort, *i.e.* one presentation of one syllable, it is shown that as the material becomes longer, it becomes relatively more difficult. Shorter materials are more rapidly forgotten than longer materials. In general, there is a decreasing susceptibility to retro-active inhibition with increasing length; but there is probably a lower limit to this law.

Discussion: The Stimulus Error, A Reply. (Samuel W. Fernberger.)

Psychological Review. Vol. xxix.

No. 6, November 1922.

Behaviour versus *Introspective Psychology.* (Samuel W. Fernberger.)

The author holds that the present data treated under the term psychology cannot be treated from a single set of interpretative categories. The science of consciousness and the science of behaviour have to be recognised as independent disciplines, and the 'older psychology' emerges as a form of speculative procedure which attempts to correlate the data of these two sciences with each other and with physiology.

The Unique in Human Behaviour. (Henry Wilson Wieman.)

Behaviour of lower animals is determined by anticipation of certain specific situations. Human behaviour is determined by anticipation of a great number of possible situations ultimately included in that total situation called the world. In other words, instincts are integrated into *interests.* A suggestive paper.

Analyzed versus Unanalyzed Experience. (Raymond Holder Wheeler.)

This is a reply to Professor Calkins' criticisms of the author's analysis of the 'awareness of meaning,' with further detailed experimental evidence. The author does not deny 'feeling of mental activity' and 'consciousness of self,' but claims that these are neither elemental nor unanalyzable; they are exclusively constituted of sensation and image. "My critics may choose either to deny the truth of introspective descriptions in which case they are denying the facts of observation, or else admit that because of a superficial introspective technique they have failed to analyze their own mental processes."

What Can Psychology Contribute to our Knowledge of the Mechanism of Mental Disorder? With some Comments on the Term 'Depression.' (C. Macfie Campbell.)

A brief discussion of the inter-relations of psychology, physiology and psychiatry, with a plea, pointed by consideration of a particular case, for the recognition of the rich complexity of the emotional life, and the danger of reducing this 'chromatic richness' of inner experience to a few simple and rigid formulae.

The Effect of Variations of the Intensity of the Illumination of the Perimeter Arm on the Determination of the Colour Fields. (C. E. Ferree and Gertrude Rand.)

By a sufficiently wide variation of the factor of intensity alone, the fields of colour sensitivity may be made to have almost any breadth in the field of vision, to differ radically in shape, and even to change or reverse their order of ranking as to breadth. The conventional clinic rating of the limits from widest to narrowest in the order of blue, red and green is a function of the relative and absolute intensities of the stimuli employed as well as of the actual distribution of sensitivities. Without great precision in the control of intensity, therefore, it is obvious that little significance can be attached to extent or shape of field. Detailed experimental results with normal subjects are given.

Types of Dextrality. (J. Merle Rife.)

It is held that if bi-manual operations are considered as well as uni-manual, three distinct types of right-handedness can be distinguished, with three corresponding types of left-handedness; and that any investigation of dextrality should always be accompanied by determination and analysis of the types.

The Significance of Neural Adjustment. (Howard C. Warren.)

It is suggested that 'neural activity' should be excluded from the connotation of *behaviour.* The question of whether imagery and thought are primarily phenomenon of behaviour or of neural activity is not yet solved. The attempt to describe organic reactions solely in terms of the initial and terminal data (stimulus and response) ignores neural adjustment, which the author regards as the key to the entire process. The individual's experience-life, including his memories and thoughts and especially the phenomena of dreams, constitutes a real class of occurrence and accordingly deserves scientific study. The behaviour method has to be supplemented by introspection and by nerve-study.

Discussion: The Affiliations of Behaviourism. (Mary Whiton Calkins.)

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING.

June 2, 1923. Some Aspects of the Philosophy of Harmonism and Future Experiments, by Sir Charles Walston.

SECTIONAL MEETINGS.

Aesthetics Section.

April 27, 1923. The Correlation of Poetry with Music, by K. M. Wilson.
May 11, 1923. The Place of Imagery in the Appreciation of Poetry, by C. W Valentine.

Education Section.

May 8, 1923. Mental Tests, by R. R. Dobson.
June 12, 1923. The Validity of Certain New Methods of Testing, by P. B. Ballard.

Industrial Section.

April 16, 1923. Rest Pauses, by G. H. Miles.

Medical Section.

May 23, 1923. Narcolepsy, by C. Worcester-Drought.
June 27, 1923. The Psycho-Analysis of Hate and Sadism, by James Glover.

JOINT MEETING.

Education and Medical Sections.

April 25, 1923. A Symposium on Delinquency and Mental Defect, by N. East, C. Burt, F. G. Shrubsall and W. H. B. Stoddart.

REPORT OF THE BRITISH PSYCHOLOGICAL SOCIETY'S COMMITTEE FOR RESEARCH IN EDUCATION[1]

ON EDUCATIONAL PROBLEMS REQUIRING INVESTIGATION BY PSYCHOLOGICAL METHODS

A. General Problems

1. *Mental Tests*

(*a*) The upper limit of the growth of intelligence.
- The time element in intelligence tests.
- The effect of practice in intelligence tests.
- The constancy of the I.Q. and the prediction value of tested ability. The relation of ability at (say) 7, 11, 13, and 18. Are there 'late bloomers'? If so, is the 'late blooming' general or confined to special functions? If it exists, to what is it due?
- Tests for supernormals. The effectiveness of the existing system of scholarship examinations in selecting supernormal children at different ages and for different purposes, with special reference to the discovery of supernormal children among the poorer social classes.
- Tests of practical ability.
- The standardisation of tests, group and individual.

(*b*) Emotional factors in results of tests; specific and general.
- 'Unconscious' factors in intelligence testing.
- Tests for emotional and temperamental characteristics.

(*c*) The relation of character to intelligence.

(*d*) Tests for specific abilities—vocational tests.

(*e*) Unit of measurement of intelligence.

2. *Tests of Attainments*

Standardisation of tests in fundamental subjects.
Standardisation of tests in secondary school subjects.
Tests of general knowledge.
Scales of marking.
Validity of older and newer methods of examining.

3. *Correlation of Mental Age and Attainments*

(*a*) For the purpose of examining.
(*b*) For the purpose of teaching—economy of time.

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[1] One of the purposes of this Committee being to draw attention to the need for research in education, the following report of educational problems which call for investigation by psychological methods has been drawn up. The Committee will be glad to give advice and assistance to those able to undertake research, and the Honorary Secretary (Mrs S. S. Brierley, M.A., Forum Club, 6 Grosvenor Place, Hyde Park Corner, S.W.) will answer any enquiries.

What aspects of a subject can be most effectively treated at a given mental age? Experiments on the results of postponing teaching in certain subjects, *e.g.* foreign languages, formal arithmetic, algebra, formal geometry.

Tests for specific abilities in relation to age of specialisation.

Tests for potential ability in respect of certain school subjects (cf. those of Dr Agnes Rogers for mathematics). For instance, is it possible to predict whether it would or would not be profitable to make a given pupil learn a foreign language?

And whether this should be done for reading or for speech purposes? At what age is the prediction first possible?

Advisability of segregating supernormals.

4. *Methods of Teaching*

Methods of teaching in particular subjects, *e.g.* reading, languages, science, mathematics.

Methods of teaching for special types of children.

Fatigue value of different subjects; over-pressure.

The most valuable length and distribution of intervals and holidays.

The relative value of formal drill and 'the play way,' *e.g.* how far is it desirable to teach geography and geometry as part of scout teaching?

What is the reaction of school-teaching on the 'creative faculties'?

Revision periods—best interval between learning and revision.

5. *The Study of Individual Children*

Long-period observations of individuals—intellectual, emotional and moral characteristics in relation to home and family conditions, school and occupation.

The following-up of children on leaving school; the study of later career in relation to school history and psychological records, and to conditions of particular occupation.

The study of particular types of children—*e.g.* delinquent, neurotic, supernormal.

The distribution and history of the 'creative faculties' (as shown particularly in the production of original literature, music, drawing, etc.). What is the curve of distribution at different ages? What is the action of self-criticism and school teaching upon these faculties? In particular, what is the proper interpretation of the apparent difference between the adolescent and the child in respect of 'creative' impulses in these regions?

How far do specific interests depend upon 'nature'? How far are they a product of 'nurture'? In particular, are there children of good natural ability, whose interests cannot be awakened by suitable school teaching? If so, why not?

The correlation of interest and ability.

An assessment of the results of adopting a vocational aim during adolescent years.

6. *Special Group Differences*

(*a*) Racial differences, mental and physical.
(*b*) Family, hereditary factors.
(*c*) Social status. The handicaps of the supernormal child from the poor home.
(*d*) Sex differences, and the relation of these to differentiation of curricula, *e.g.* How far (if at all) does the natural *tempo* of intellectual development differ in the sexes?

7. *Group Psychology*

(*a*) In relation to class instruction.
The appropriate place of class and individual methods of instruction.

(*b*) In relation to discipline.
The influence of different types of discipline on different types of children.
Incentives (including punishment).

8. *Psycho-analysis*

Its educational bearings. (*a*) The educational value of the psycho-analytic technique. (*b*) The knowledge of child psychology resulting from the technique. The relation of psycho-analytic theories to the problem of discipline.

The social development and education of children under eight years of age; and of adolescents. Evidence for the repetition of infantile phases in adolescence.

The incidence of psycho-neurosis in childhood.

The dreams and fantasies of individual children in relation to the problem of the creative imagination and to the emotional problems of school and family life.

B. More Special Problems

(1) The functional value of visual and other imagery in relation to school subjects, especially those involving reasoning.
In particular the possibility of the useful organization of imagery (*e.g.* of 'number forms' in recording for reproduction the results of manipulating numbers).

(2) The alleged differing reactions of boys and girls towards specific subjects (*e.g.* mathematics, geography, mechanics). (Mr Fairgrieve has collected data for geography by a careful scrutiny of examination answers.)

(3) 'Sense-preference' in learning, how far is it natural, how far due to accidents of teaching?

(4) A census of spelling errors and of syntactical difficulties at different ages.

(5) The value of different types of 'composition' in training written expression; dependence of that value upon psychological type of the pupil.

(6) Specific studies of the results of 'project' methods (as contrasted with formal teaching) as regards accuracy, permanence and availability of knowledge. (The early stages in teaching reading, writing and arithmetic would be included.)

(7) (Connected with 6.) The accuracy, permanence and availability of knowledge acquired 'incidentally' (*e.g.* by reading and writing, of the grammar of English and other languages).

(8) An experimental examination of the basal assumption of the 'direct' method of teaching a foreign language.

(9) Specific studies of the development of the power to interpret and use symbolism (*e.g.* maps in geography, algebraic formulae). An analysis of the functional value of different types of symbolism (*e.g.* graphic as contrasted with algebraic methods in teaching mathematics).

(10) Detailed studies of children's reactions to specific teaching methods (cf. Dr Ballard's and Mr Winch's inquiries in arithmetic).

(11) The effect of rest intervals (*e.g.* dropping a subject for a term) upon progress in school subjects.

(12) Studies of the loss of the results of school teaching after leaving school.

(13) Experiments upon the reliability of brief methods of examining knowledge (*e.g.* the 'missing word' method).

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THE CONCEPTION OF NERVOUS AND MENTAL ENERGY[1] (I)

BY E. D. ADRIAN.

I HAVE been asked to discuss the conception of nervous and mental energy as it appears to a physiologist whose chief concern is with the nerve fibre rather than the brain. At present I do not think that the physiology of nervous conduction has advanced far enough for its results to be of any real significance for the psychologist (except in so far as he studies the physiology of the sense organs); speaking from a purely physiological point of view, it seems to me that the less we say about nervous and mental energy the better.

The difficulty arises over the meaning of 'energy,' for we seem to use the word in two very different senses, the biological and the physical. We may say that *X* is a man of enormous energy, or that the energy possessed by a moving body in virtue of its movement is $\frac{1}{2}mv^2$. In both phrases we are using 'energy' to mean the capacity to do work, but in the first case work means biological or 'purposive' activity (for want of a better description), and in the second it means physical work, the product of a force into a distance. No doubt we have a perfect right to speak of nervous energy or mental energy in the biological sense. The physical sciences cannot claim to be the only branches of knowledge which have a right to the term 'energy,' but they have certainly made it so precise and full of meaning that it is difficult for us to ignore this meaning completely. The energy which forms the subject matter of thermodynamics involves notions of mass, length, etc., all of which can be applied only to material systems. If we speak of 'mental energy' (to take the extreme case), we may mean something analogous to physical energy, but it is obvious that it must be defined in quite different terms. Mental work, and mental forces and resistances may have a clear enough meaning for us, but it is a meaning which involves ideas of life and purpose. Mental energy may perhaps be regarded as the product of the force into the resistance overcome (though both are imponderables), but because there is an analogy there is no reason to suppose that this mental energy will follow any of the generalisations which have been observed for the energy changes in material systems. These generalisations, particularly the first law of thermodynamics, have become so much part and parcel of our ideas about energy that we are in some danger of forgetting that they are concerned only with the problems of thermodynamics and not with those of behaviour. How far this danger is a real one is not for me to say, but I seem to have come across passages in psychological literature which suggest some confusion, particularly in semi-popular explanations of the theories of Freud.

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[1] Read as part of a Symposium at the Oxford Meeting of the 7th International Congress of Psychology, 30 July 1923.

The same objections apply to the term 'nervous energy,' when we use it in a biological sense. If we say that a neurasthenic's store of nervous energy is very small, we mean that he is incapable of sustained purposive activity; we do not mean that his brain and spinal cord, if burnt in a calorimeter, would yield less heat than those of a healthy person. Yet we may easily make the mistake of assuming that the law of the conservation of energy must hold good for this 'nervous energy,' so that an increase in activity of one kind must necessarily involve reduction in another kind. This may be true, but it does not follow from the laws of physics. For this reason, although 'nervous energy' in the biological sense may be a convenient term for the neurologist, I, for one, should like to banish it from our vocabulary and to substitute some other phrase which could be defined afresh and would not carry with it these physical implications.

The same objections do not apply at all when we use 'nervous energy' to mean a special manifestation of physical energy peculiar to the living nerve fibre but transferable into other forms of energy in accordance with the laws of thermodynamics. How far we are likely to regard 'nervous energy' in this sense as a valuable conception will depend on the direction from which we are trying to approach to an understanding of the nervous system. MacDougall's hypothesis of 'neurin,' the fluid conveying energy from one part of the nervous system to another, has shown itself of value in correlating the phenomena of inhibition, fatigue, hypnosis, etc., but, as he points out, it is more welcome to the physician and the psychologist who deal with the organism as a whole than to the physiologists of the laboratory who are chiefly concerned with the separate organs. For these the nervous system must be regarded as a complex system of nerve fibres, cells and synapses, and its reactions must be interpreted in terms of our knowledge of the activity of these constituents. Now we know something about the activity of nerve fibres when they have been removed from the body and placed in artificial surroundings. In this case the result of a local stimulus is the development of a nervous impulse or a succession of impulses which travel rapidly down the fibre in either direction. Each impulse is a momentary change which shows itself chiefly by the development, in the active region, of an electric current brought about by a change in the surface layers of the fibre and a movement of ions into the fluid round it. The exact details of the process remain to be worked out, but it seems to have an understandable physics and chemistry, and there is nothing to be gained by assuming that the energy degraded in the passage of an impulse exists at any stage in a peculiar form unlike the manifestation of energy in non-living systems.

So a special nervous energy is not wanted to explain the nervous impulse; but we have still to ask whether the transmission of this kind of impulse is the only form of activity of which the fibre is capable when it is in the body. The answer to this question is necessarily uncertain. In the isolated nerve trunk we cannot detect any other kind of propagated disturbance, anything in the nature of a continuous rather than an intermittent activity, but this may be because we have not found the right kind of stimulus. In the body there are some indications that the tonic postural contractions are maintained by a steady shortening of the muscles rather than a fusion of successive twitches, and this steady shortening may be produced by a different kind of nervous activity. But the rapid movements of the limbs, whether voluntary or reflex, are undoubtedly due to a series of twitches in the muscles such as would be produced by repeated stimulation of the nerve and by a succession of impulses travelling down it. In the case of the movements of breathing, Gasser and Newcomer have shown that there are electric oscillations in the phrenic nerve corresponding to the twitches in the diaphragm muscle and of the same form as the electric oscillation in an isolated nerve stimulated artificially. Forbes and Gregg have shown that impulses of the same kind travel down the motor nerve of the arc concerned in the flexion reflex when the sensory nerve is stimulated electrically or mechanically, and this has been amply confirmed in later work.

There is only one set of results which suggests that the nervous influence is continuous and not a succession of impulses, and these are the results obtained by local alterations of temperature. Observations of this kind were first made by Miss Buchanan in the Physiological Laboratory at Oxford, and they showed that the frequency of the oscillations in the muscle depended on the temperature of the muscle and not on that of the spinal cord. The result is puzzling, and several explanations have been put forward. During the past year some work I have been doing with Miss Cooper on the central nervous system has made it imperative to try and decide what is really happening in the motor nerve, and we have repeated these and similar experiments on frogs and mammals. The results leave no doubt in our minds that the reflex contraction of a muscle is produced by a series of impulses travelling down the motor nerve, and not by a continuous influence of any kind, though it would take too long to explain how we have arrived at this conclusion.

There is another piece of research, published last year, which supports very strongly the view that the work of the nervous system is carried out by succession of impulses of the same type as those we study in isolated nerve. This is Brücke's work on inhibition. Keith Lucas suggested that an inhibitory stimulus might take effect on the central nervous system by producing an increase in the frequency of the impulses travelling to the motor centres. The consequent overcrowding of the impulses would make them smaller, or less able to travel, and if their path to the motor centre included a region of imperfect conduction a rapid series of impulses might fail to pass this region, although a slower series would have no difficulty. Brücke has stimulated two different nerves simultaneously, the nerves being chosen so that the effect of one is to excite and of the other to inhibit a reflex contraction, and he finds the inhibitory effect at a maximum when there is a certain phase relation between the two sets of stimuli. This relation is of exactly the kind which could be expected if the inhibition depended on an overcrowding of impulses in the path leading to the motor centre.

In a recent review[1] Professor Forbes of Harvard has discussed the various difficulties which arise when we attempt to explain the reactions of the central nervous system as depending entirely on the passage of impulses from the afferent to the efferent side, and he concludes that none of these difficulties is insuperable. In a scheme of this kind the conception of nervous energy can be of very little help. It is not wanted to explain the nervous impulse and we know so little of any other kind of nervous activity that we have no claim at all to postulate a special kind of energy to explain it.

There is one other way in which the phrase 'nervous energy' can be used, and this was hinted at in a letter to *Nature* by Professor Fraser Harris last year. Most people will understand what we mean when we say that there is a widespread discharge of nervous energy during asphyxia or strychnine poisoning. We mean that widespread and forcible muscular contractions are produced through the agency of the central nervous system. There is something to be said for this usage, for it does not commit us to any theory of nervous activity. If we accept the view that contractions are caused by a succession of impulses in the motor nerve fibres, we are really using 'a discharge of nervous energy' as synonymous with 'a discharge of nervous impulses,' but our meaning will be fairly clear even if we suppose that the nerves act in some other way. At the same time, the use of 'energy' still seems to me to be open to misconception. 'Nervous activity' conveys exactly the same idea without suggesting the laws of thermodynamics, and although the dangers of confusion are small, they are surely worth avoiding.

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[1] *Physiol. Rev.* 1922, II, 403.

The arguments I have brought forward could really have been stated in a few lines. They amount to this: if we use the word 'energy' in its purely physical sense, the conception of nervous energy is unnecessary, and that of mental energy is impossible. If we use it in another sense, we must be careful to define its meaning exactly or we run the risk of assuming that it must necessarily follow the rules which have been found to govern the transformations of physical energy in material systems. But it would be foolish of me to suggest that psychologists as a whole are not alive to these risks, and I am quite ready to believe that the conception of mental energy, properly defined, may be as necessary to psychology as that of physical energy is to physiology.

THE CONCEPTION OF NERVOUS AND MENTAL ENERGY[1] (II)

('VIGILANCE'; A PHYSIOLOGICAL STATE OF THE NERVOUS SYSTEM)

BY HENRY HEAD.

1. *What is meant by 'vigilance'?* (pp. 127–133).
2. *Automatic actions* (pp. 133–135).
3. *Sensation and its subservient physiological dispositions* (pp. 135–137).
4. *The continuity of consciousness* (pp. 137–138).
5. *Purposive adaptation* (pp. 138–140).
6. *Psychical and somatic behaviour dependent on the state of 'vigilance' of the nervous-system* (pp. 140–145).
7. *Summary* (pp. 146–147).

THE neurologist grows so familiar with the various forms of loss of function, produced by an organic lesion of the central nervous system, that he is liable to consider them all in anatomical or regional terms. Motor paralysis, defective sensibility, or disorders of speech seem to him to be equally explicable, provided he can discover the exact limits of the destruction of tissue to which they are due. He is liable to forget that, between these structural defects and the affection of sensibility or of speech, lie intermediate vital processes of profound importance. These have a determinant action on the form assumed by the manifestations. The effect of the structural changes does not depend only on their extent and severity; the mode and rapidity of onset and the general condition of the nervous system at the time are of even greater importance in determining the nature of the loss of function.

Now there is little difficulty in correlating a motor paralysis with destruction of cells and fibres in the spinal cord or even in the brain; we know that, so long as they live, they govern the function of muscular movement. Even loss of sensation seems easily explicable; for the neurologist is liable to consider mainly the destination of afferent impulses and to ignore the fact that sensation is a process of mental activity. So many of the duties exercised by afferent impulses are concerned with control and guidance of motion or other acts, which are performed outside consciousness, that the essentially psychical nature of sensation is neglected or forgotten. To many 'afferent' and 'sensory' are equivalent terms.

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[1] Read as part of a Symposium at the Oxford Meeting of the 7th International Congress of Psychology, 30 July 1923.

But, when we consider disorders of speech, the mental aspect of the loss of function cannot be ignored. There are consequently three factors, which equally demand recognition. First, the extent and nature of the anatomical destruction; secondly, the physiological disturbance it evokes, and lastly, the nature of the psychical manifestations we speak of as aphasia.

It is the second term of this causal chain that is habitually slurred over. The loss of tissue can be determined by anatomical examination and the clinical manifestations are patent to anyone who will take the trouble to observe them; but no intervening link is discoverable between them. Few to-day go the length of those stiff-necked anatomists who assert that single words are localised in single cells or cell complexes; or that each psychical group of functions possesses an independent anatomical substratum, formed of certain cortical centres in combination with appropriate association paths. Yet, few have attempted to consider the steps by which a structural change evokes a physiological disorder which in turn is manifested as some defect of a specific psychical process.

We are accustomed to associate a violent injury to the head with loss of consciousness and other symptoms of concussion; empirically we are equally familiar with that curious unwitting state, which precedes the patient's complete recovery, during which he is liable to act apparently reasonably, but in a purely automatic manner. But we make no attempt to explain how a localised lesion of the brain can abolish certain specific mental activities, leaving other independent functions of the mind intact. In reality, all these phenomena are aspects of the same problem, though familiarity with the facts has staled the wonder they ought to excite within us. For in the phenomena of aphasia, we are face to face with the relation of body and mind in a form capable of experimental examination, and we are compelled to ask ourselves how a diminution in physiological activity can be associated with specific psychical defect.

1. WHAT IS MEANT BY 'VIGILANCE'?

But before we can attempt to answer this question it is necessary to take into consideration certain facts in the physiology of the central nervous system.

When the spinal cord is divided, or so grossly injured in man that conduction is destroyed, the lower extremities lie flaccid and atonic on the bed in any position into which they may be placed. The urine is retained, the patient has no power of evacuating his bowels, and at first the skin is dry. All deep reflexes are abolished, and scratching the sole of the foot may either produce no movement of the toes, or one that is feebly downwards.

Should the injury be acute and the patient young and otherwise healthy, particularly if he remains free from cystitis, bed sores or fever, the deep reflexes reappear, as the period of spinal shock passes away. First the ankle jerk, and then the knee jerk can be obtained; gradually the plantar reflex begins to assume a form characterized by an upward movement of the great toe. The field from which it can be evoked enlarges and finally, in successful cases, the spinal cord becomes so excitable that stimulation anywhere below the level of the lesion may be followed by a characteristic upward movement of the toes. But this now forms a small portion only of the reaction to superficial excitation; ankle, knee and hip are flexed and the foot is withdrawn from the stimulus applied to the sole. Not infrequently the abdominal wall is thrown into contraction and every flexor muscle below the lesion may participate in an energetic spasmodic movement. Stimulation of a small area on the foot has evoked a widespread response from the whole extent of the spinal cord below the lesion[1].

Still more remarkable evidence of this diffusion of afferent impulses in the isolated cord is given by the behaviour of the bladder and rectum. For a considerable period after the injury, the patient is unable to pass either urine or faeces; but, in favourable cases, this merges into a condition where both bladder and rectum are emptied automatically. At this stage of excitability, evacuation can be facilitated by scratching the sole of the foot or by any manipulation, which produces a flexor spasm. At the same time, passing fluid into the bladder or rectum may evoke an upward movement of the great toe, accompanied by other characteristic flexor manifestations of the lower extremity[2].

This condition has been called a 'mass-reflex,' because any afferent excitation below the level of the lesion is liable to produce motor effects, not only in the parts normally thrown into action by such a stimulus, but also in organs that do not usually lie within the arc of its reflex influence. The bladder can be excited to action by stimulating the sole of the foot, and movements of the toes can be evoked by filling the bladder with fluid.

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[1] Riddoch, G. *Brain*, 1917, XL, 264–402.

[2] Head, H. and Riddoch, G. *Brain*, 1917, XL, 188–263 and *Neurological Studies*, 1920, II. 467.

Once this condition has been established, the mechanism by which it is produced may become astonishingly sensitive, and comparatively slight stimuli applied to any part below the lesion may evoke violent and widespread movements. But if the patient develops fever, in consequence of toxic absorption from a bed sore or septic bladder, his condition may fall back to that found shortly after the injury. The deep reflexes disappear, the field for the plantar response becomes restricted to the sole or lower portion of the leg, and evacuation of bladder and rectum can no longer be facilitated by stimulating the lower extremities or surface of the abdomen. Not only has the excitability of the cord diminished, but the nature of the response to stimulation reverts to its earlier condition. This may also occur after some operative procedure carried out within the spinal canal, especially if the dura mater is opened. Even a gastro-intestinal disturbance, unaccompanied by fever, may produce the same signs of lowered activity.

But, if the toxaemia or other abnormal state passes away, the isolated spinal cord gradually regains its power of responding vehemently to afferent stimuli and the mass-reflex may be evoked as vividly as before. But anything which tends to lower the vitality of the isolated spinal cord will diminish these manifestations of high neural potency.

These observations on man are in complete agreement with the animal experiments of Sherrington[1] and his pupils[2]. Suppose, for instance, that the spinal cord of a cat has been transected in the region of the medulla oblongata; twenty minutes later prick the hind paw with a pin and no general reflex results, but the toes make an opening movement. Gradually the response becomes more widespread, until the whole of the limb may be thrown into flexion and the opposite one extended by a stimulus of the same nature and intensity. Not only has the motor response become brisker and more extensive, but the skin area from which it can be evoked has greatly increased. Pinching the superficial structures over any part of the limb may now cause flexion, accompanied by extension of the opposite extremity.

The deep reflexes reappear rapidly and the character of the knee-jerk shows that the quadriceps has regained tone to a considerable extent. As the spinal preparation improves in excitability, even the scratch reflex may reappear; it is, however, easily fatigued and the foot is not brought up to touch the spot stimulated on the body.

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[1] Sherrington, C. S. *The Integrative Action of the Nervous System*, 1906.

[2] For the opportunity of seeing these experiments and of watching the effect of chloroform in the spinal and decerebrate preparation, I am indebted to the kindness of Sir Charles Sherrington. I also wish to express my thanks to Professor Bazett, who was then working in the physiological laboratory at Oxford, for showing me his animals and giving me much information about them (Bazett, H. C., and Penfold, W. G. *Brain*, 1922, XLV, 185-265).

When the spinal cord has reached this high condition of activity, the administration of chloroform causes rapid regression. Knee-jerk and ankle-jerk disappear and finally the only reflex that can be evoked is a slight movement of the toes, elicited from the pad of the foot only. Pricking any other part of the limb no longer produces any effect.

Let the preparation recover and, as the narcosis passes away, the receptive field enlarges rapidly; full flexion, with contralateral extension, is again evoked by pricking any part of the limb. At the same time knee- and ankle-jerks reappear; at first they consist of a short twitch, but, as the muscles recover their spinal tone, these reflexes regain their previous character.

It is obvious, therefore, that, even when completely transected, the spinal cord is capable of various degrees of activity, manifested not only in a brisker reaction, but also in different forms of reflex response. A toxaemia, or anything which lowers vitality, tends not only to diminish the response, but to change its nature. This may be temporary and cannot be due to gross structural disorganization, for it can be produced in a purely transient form by the action of chloroform. Here we are face to face with a profound coming and going of function in the completely isolated spinal cord, associated with certain vital or physiological conditions, which cannot be caused by gross anatomical changes.

This is still better exemplified in the decerebrate preparation[1]. If the head is removed just in front of the anterior colliculi the cat is capable of a large number of discriminative reflexes. Water placed in the pharynx is swallowed in a normal manner, and, when a small quantity of alcohol is added, the tongue makes a wide sweeping movement, curling at the tip as if to lick the lips. On the other hand, oil produces no movements of the pharynx or tongue. Several different reflexes can be evoked from the pinna, each of which is adapted to the nature of the stimulus; if it is pricked it folds down, but touching the hairs within the ear causes a movement, which would be most effective in dislodging a flea. A drop of water placed in the pinna is followed by a rotation of the head, which brings the stimulated ear downwards, followed by a rapid shake admirably adapted to displace the fluid.

Sometimes a preparation of this kind can purr; it will respond vocally in an appropriate manner to the singing of a canary or the bark of a dog, and at the same time the tail makes the movements customary with such states of excitement.

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[1] Bazett and Penfold, *loc. cit.* 214.

Normally a decerebrate animal is rigid in extension and if balanced can support its own weight; that is to say, it can stand with help. It sits up with head erect, the forepaws stretched out on either side and the hind limbs tucked beneath it. But if it is put aside in a quiet place, and, after an interval, is approached with great care, it may be found lying on its side relaxed, with its limbs flexed as if asleep. Jarring contact, or even a slight noise, provided it is capable of responding to auditory stimuli, will cause it to assume its natural state of rigid extension. It has woke again from the reflex relaxation proper to sleep.

Hold up such a preparation so that it stands on its four limbs, or place it in a seated posture on the table and then administer chloroform. It will sink together exactly like a normal animal; the head falls forwards, the legs double up and the tail ceases to curl at the tip. Then the reflexes become diminished and the knee-jerk loses its characteristic decerebrate form; it becomes a simple jerk, like that of a spinal animal, unaccompanied by increase in extensor tone. The pinna reflexes cease to exhibit their diverse and appropriate forms; swallowing is no longer produced by a drop of water in the pharynx and even alcohol evokes no response.

So long as spontaneous respiration continues, the limbs do not become completely flaccid; but if chloroform is pushed until breathing ceases and artificial ventilation becomes necessary, even the knee-jerk may disappear and all postural tone becomes abolished in the hind limbs.

At this stage, remove the chloroform, but continue artificial respiration. As the effects of the anaesthetic wear off, postural tone is restored, spontaneous breathing returns, swallowing is again possible, the pinna reflexes become differentiated according to the form of the stimulus, and the knee-jerks recover their plastic character. Finally, the preparation may return exactly to its original condition. This regression and restoration of function under the influence of chloroform can be watched time after time in the same decerebrate animal, if care is taken not to abolish the heart-beat.

The spinal and decerebrate preparations differ profoundly from one another in their reactions to external stimuli; but, in both alike, the nature of the response varies greatly according to the general state of vitality in the particular part of the central nervous system, which has been isolated from higher control. The transected spinal cord of man can live and function on a level characterized by low grade reactions; but, under favourable conditions, its activity may increase, until there is scarcely a part below the lesion from which a movement of the toes and a flexor spasm cannot be evoked or evacuation of the bladder and rectum facilitated. The vigour of such a response varies from day to day and is diminished by any state, which lowers the general vitality of the isolated spinal cord.

In the same way the spinal cat at first yields reflexes of a low order; but, with the improvement in its condition, the most complicated movements can be obtained involving mutual inhibition of flexion and extension. The animal may even regain the scratch reflex in an incomplete form. On administering chloroform all these reactions disappear, to be again recovered as the influence of the anaesthetic passes off. In man this degradation can be produced by a temporary toxaemia, and the restoration of the various forms of response can be watched step by step during recovery from its evil effect.

Now, it is scarcely sufficient to say that these phenomena are manifestations of heightened or lowered excitability. If we say that some part of the nervous system is in a state of raised excitability, we mean that it responds in the same manner to a stimulus of diminished strength. The excitability of a tissue is measured by the ease with which it can be provoked to activity by a suitable external stimulus and is therefore inversely proportional to the strength of the excitation needed to produce any recognisable sign of activity, in fact to its threshold value[1]. But, in the phenomena we are discussing, the main factor is not the diminution or increase in the strength of the stimulus, which is required to evoke a reaction, but the fundamentally divergent nature of the response, according to whether the preparation is in a high or low state of vitality. At one time it is not only more prepared to receive and react to external impressions than at another, but an identical stimulus produces a different result and, during a high state of vitality, tends to evoke more of less high-grade adapted movements.

These reach a marvellous complexity and perfection in the decerebrate animal. Here the nature of the movement of ear or tongue depends on

[1] It is sometimes used with a meaning more akin to the German *Leistungsfähigkeit*. It then signifies the responsiveness of the tissue to a given stimulus and would be measured by the strength of the response, *e g.* the height of contraction in a muscle evoked by a stimulus above the threshold value. This is misleading. It is true that a tissue which gives a large response can often be activated by a weak stimulus, but there is no invariable correlation between the value of the threshold stimulus and the size of the response. In fact, there are many cases where a raised threshold is associated with a relatively increased reaction; once a stimulus is sufficient to evoke a response the effect produced is both greater in amount and more extensive than under normal conditions.

the form and quality of the stimulus rather than on its strength. The response is truly discriminative, but can be abolished by any condition inimical to a high state of neural vitality, such as sepsis, haemorrhage into the wound, or the lighter degrees of chloroform narcosis. Apart, however, from such hostile influences, the decerebrate preparation may occasionally be found with its limbs in a state of flexion, as if asleep; any contact, or in some instances even a noise, will rouse it and all four extremities at once reassume the characteristic posture of tonic extension. If we were dealing with an intact animal, we should say that these differences were associated with the presence or absence of consciousness.

But such an explanation of the reactions of the spinal or decerebrate preparation is unsatisfactory, and I would suggest that we employ the word 'vigilance' for this state of high-grade physiological efficiency. Shortly after division, the spinal cord of man is in a condition of low vigilance. It is still excitable, for scratching the sole of the foot may produce a movement of the toes. Under favourable conditions the vigilance of the cord increases, until at last it is in a state of intense readiness to respond to excitation. In the cat the reaction may now assume the form of a series of complex movements, involving both lower extremities and necessitating coordinated excitation and inhibition. The tone of the flexor muscles has increased profoundly and, even in the extensors, has become sufficient for reappearance of a knee-jerk.

As revealed in the decerebrate preparation of Sherrington and his fellow-workers, vigilance is expressed in heightened extensor postural tone and acutely differentiated responses. This high state of physiological efficiency differs from a pure condition of raised excitability; for although the threshold value of the stimulus is not of necessity lowered, it is associated not only with an increased reaction but with highly adapted responses. These may vary profoundly according to circumstances, which are not inherent in the nature of the stimulus.

2. AUTOMATIC ACTIONS.

If we did not know that the whole of the brain had been removed, we should say that the actions of the decerebrate animal were directed by consciousness. It initiates no movement spontaneously, but purposive adaptation is evident in every response. The character of its purring varies with the nature of the auditory stimulus; irritation of the ear excites a variety of highly differentiated movements, each of which is accurately designed to the end in view. Thus, every one of these actions may be purely automatic. Their formal recognition, if present, would be accessory to a mode of behaviour, which can be carried into execution without it.

Both the decerebrate preparation and the normal cat react in an equally discriminating manner, when a flea or a drop of water is placed in the ear. The individual significance of each stimulus is registered, and suitably adapted responses are prepared on a purely physiological level. But, whenever a reaction shows these characteristics, it demands a high state of vigilance in those parts of the nervous system necessary for its performance. The more highly differentiated the act, the greater degree of vigilance does it require and the more easily can it be abolished by toxic influences, such as chloroform, or by other conditions unfavourable to physiological activity. Experimentally we can watch the various responses disappear step by step under the influence of the narcosis.

Many actions acquired during the life of the individual tend to be carried out on the physiological level. A child has to be taught not to wet the bed at night, and this control of a spinal reflex becomes so completely automatic, that it is maintained even in the deepest sleep. But anything which lowers the vigilance of the central nervous system, such as dyspnoea or a debilitating illness, is liable to disturb this control and the bed is again wetted at night.

The same law is illustrated in many aptitudes, such as flying, shooting and out-door games, which are acquired and maintained by conscious effort. They never reach a high degree of perfection until the necessary movements are carried out unwittingly. The counter stroke of an expert lawn-tennis player is determined at the moment, when the ball leaves the opponent's racquet, whilst the tyro waits until it has bounded on his side of the net; a good shot watches the bird and pays no attention to his gun. Moreover, anything which at the moment concentrates the mind on the various stages of a mechanical action, diminishes the likelihood that it will be carried out successfully. The performer must concentrate on the goal or intention of his desires, and trust to habitual skill for its mechanical execution. But all these aptitudes are profoundly influenced by conditions of general health or by anything which lowers physiological vitality. A common cold, a gastro-intestinal attack or even mental worry may materially diminish any form of mechanical skill.

Every automatic act is an exercise in physiological memory and can be disturbed by vital states, which have nothing to do with consciousness. What wonder that the complex powers, demanded by speech, reading and writing, can be affected by a lesion, which diminishes neural vitality. Vigilance is lowered and the specific mental aptitudes die out as an electric lamp is extinguished, when the voltage falls below the necessary level. The centres involved in those automatic processes, which form an essential part of the conscious act, may continue to live on at a lower vital level, as when under the influence of chloroform; they do not cease entirely to function, but the vigilance necessary for the performance of their high-grade activities has been abolished by the fall of neural potency.

3. SENSATION AND ITS SUBSERVIENT PHYSIOLOGICAL DISPOSITIONS.

The changes produced by a cerebral lesion in all forms of sensation yield striking proof of the wide range of physiological processes underlying the more limited field of specific consciousness. Thus, before the afferent processes caused by the movement of a joint can evoke a sensation, they must be integrated and brought into relation with physiological dispositions, which are the result of antecedent changes in posture. Complete appreciation of spacial relationships is recognition of serial changes in a certain direction. On a taximeter the measured distance we have travelled is presented to us already translated into shillings and pence; so the final product of spacial changes rises into consciousness as a measured postural appreciation.

For this standard against which all subsequent changes in posture are measured before they appear in consciousness we have proposed the word 'schema'[1]. By means of perpetual alterations in position we are always building up unwittingly a model of ourselves, which is constantly changing. Every new posture or movement is registered on this plastic schema and the activity of the cortex brings into relation with it each fresh group of afferent impulses, evoked by a change in the position of the body. The psychical act of postural recognition follows, as soon as this relation is completed on levels that are not associated with consciousness.

The only constant and continuous record of our movements in space exists in the condition of these schemata. For innumerable changes in posture occur, which are not represented in consciousness, and, were it not for these physiological dispositions we have called schemata, the inception of a voluntary movement would often find the part to be moved in an unsuitable attitude. Unless postural impulses continuously modified these unconscious activities in consonance with every change in bodily attitude, we might will a movement that was impossible owing to the situation of the limb. This is evident in every case of ataxy.

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[1] Head, H. *Neurological Studies*, 1920, 605 and 722. Head, H., and Holmes, Gordon. *Brain*, 1911–12, xxxiv, 186.

In order that any part of the body may be able to spring off, like a runner, at the word of command, its postural tone must be normal. Should it be posed atonically the voluntary act has to gather in slack before the limb can begin to move. Normal posture and normal tone are coincident terms.

Any lesion, which disturbs postural schemata, will interfere with static tone. For in order that any part of the body at rest may retain a normal posture, afferent impulses must exert a constant constructive influence on postural schemata. The physiological state brought about by this stream of afferent impressions not only checks and controls voluntary movement, but insures that the static tone of the part shall be adapted to maintain its position. Consciousness is in no way necessary for such coordination; in fact, tonic innervation, though dependent on afferent impulses, is determined and regulated entirely on the physiological level. Any lesion, which destroys postural schemata, may diminish static tone, if the other conditions are suitable. It will only affect sensation, when the want of these spacial dispositions disturbs the activity of some centre habitually associated with conscious processes. For instance, if the vigilance of the so-called sensory cortex is lowered, either temporarily or in consequence of organic destruction, not only will postural sensibility be disturbed in the appropriate parts of the body, but their static tone will be diminished. This is not due to the loss of postural sensations, but to the destruction of those physiological processes on which they are based. For static tone is automatic and depends on a constant stream of influence exerted by the action of ever-varying postural schemata.

When the highest centres, though themselves intact, no longer receive normal afferent impulses, the affected part of the body seems "numb" and the patient complains of "loss of feeling." But, if the schemata are destroyed or eliminated by a lowering of physiological activity, the limb or some portion of it disappears from consciousness.

The following case is an instructive illustration of these principles. A patient of mine had received a gun-shot injury, which had disorganized the elbow joint and completely destroyed the ulnar nerve. For five months we hoped to be able to save the limb, but were at last compelled to amputate through the lower third of the arm. From the time he was wounded, there were the usual changes, both motor and sensory, associated with complete ulnar paralysis; but the little finger alone was devoid of all forms of sensibility, superficial and deep. So long as he retained his limb, this finger seemed to him a dead object attached to the hand. But the phantom hand, which appeared after amputation, had four digits only. During the five months of total insensibility, the schemata associated with the little finger, no longer reinforced from the periphery, had gradually died away and, when the actual hand was removed, this digit was absent from consciousness. A portion of the body, cut off from the central nervous system, but attached to structures endowed with sensibility and movement, may continue to exist as a 'dead' part of ourselves. Like a dental plate in the mouth, it occupies a certain place in our spacial conceptions. But, as soon as the structures on which it is based are removed, it disappears from consciousness, whilst the normal parts of the amputated limb are represented in phantom form.

4. THE CONTINUITY OF CONSCIOUSNESS.

In all discussions on the relation of body and mind stress is laid on the continuity of consciousness. But this continuity is produced by habitually ignoring the gaps. However long the period of dreamless sleep or chloroform narcosis, the processes of the mind seem to have been uninterrupted. A sufferer from minor epilepsy may become completely unconscious and yet remain ignorant that he has had an attack; the stream of mental processes seems to him as unbroken as that of the astonished spectators. But during this period of unconsciousness he can carry out the most elaborate acts, guided and governed by what appear to be reasonable though extraordinary motives.

The mind, acting as a whole, not only ignores these gaps in general consciousness, but habitually neglects many possible experiences even on some specific level, such as that of sensation. The normal individual does not recognise that he is colour-blind over a considerable area of his retina. Close one eye, so as to exclude the compensating effect of binocular vision, and the world around seems unchanged. If, whilst we fix an object in front, someone then brings a red or green square of suitable size from behind forwards. it appears first of all to be either dark or light grey, although otherwise clearly visible. It will have lost its colour; but so little are the peripheral parts of the visual field concerned with this aspect of sensation, that we fail to notice its absence. In the same way we ignore the blind spot, even in uniocular vision.

Still more remarkable are the conditions found with an homonymous hemianopia of cerebral origin. The patient knows nothing of any defect in his visual field. A man, who has lost an eye or suffers from destruction of the optic nerve, recognises that he is partly blind; but one, who is deprived of sight over one-half of the field from cerebral causes, is ignorant of any abnormality in his vision. He wonders that he so frequently collides with objects on one side rather than on the other and cannot understand why they seem to disappear prematurely in this direction.

Such abolition of a specific form of psychical activity, without leaving a trace on consciousness as a whole, may occur as a temporary phenomenon. After injury to the brain a slight Jacksonian seizure, a severe bout of headache, or even fatigue and worry may be accompanied by profound loss of somatic sensibility. But this disturbance of function appears in consciousness as absence of the hand or some part of the limb. It does not seem "numb" or devoid of sensation; it simply ceases to exist. With recovery from this temporary condition of lowered vigilance in the nervous centres the lost part returns to consciousness and the patient may then recognise that its sensibility is abnormal, complaining that "it feels wrong."

5. PURPOSIVE ADAPTATION.

Purposive adaptation is sometimes said to be a distinctive mark of mental activity. But if we analyse this mode of behaviour, its essential elements are found to a varying degree in reactions, whether conscious or not, from all parts of the central nervous system.

It depends fundamentally on three factors. The character of the response must vary with specific differences in the quality of the stimulus and not only with its relative amount. Secondly, the form assumed by the reaction is determined more or less by the sequence of events and is not dependent solely on the excitation of the moment. Lastly, the spacial elements, present in afferent impulses, lead not only to coordination of muscular movement, but to projection of qualitative differences on to those external objects, which evoked them.

Once a stimulus applied to a peripheral nerve is effective, the result is the same in kind, varying in amount only. But, in the spinal cord, diverse forms of stimulation tend to produce specific and individual reactions; these may be mutually antagonistic and, should they clash, are not summed, but one or other may be inhibited. As we ascend the nervous system this mode of adaptation becomes developed to an even finer degree; the decerebrate preparation, for example, exhibits purposive responses of a very high order in harmony with differences in the nature of the stimulus. For a flea in the ear has a different significance from a drop of water, and behaviour varies accordingly. Thus, even in these lowly circumstances, certain characters of an external event are accepted whilst others are ignored and the object acquires a 'meaning' for the reacting organism. Differential selection occurs from amongst the massive presentations and this determines the form assumed by the response.

The second factor, which plays a part in purposive adaptation, even on the physiological level, is the influence exerted by past events on the reaction of the moment. The central nervous system does not function on the principle of an automatic machine, where a coin thrust into one slot produces chocolates, into another a box of matches or some other article. What we obtain, when we throw a stimulus into some part of the central nervous system, depends to a great extent on what has happened before. A neural response of this order is a march of events and not a series of disconnected episodes; the past is active in the present, and within it lies implicit the form of future reactions. Here we find the germ of the conscious processes of memory and intention.

The third element, which renders possible purposive adaptation on a physiological level, is comprised under the term projection. Afferent impulses fall into two groups; if consciousness is present, one set arouses qualitative sensations and affective states, whilst others are concerned with finer discrimination or with diverse forms of spacial recognition. The latter coordinate the motor side of the response and render it in this way effective; under their guidance the scratch reflex is exactly adapted to remove the source of irritation in the flank. These spacial afferent elements also enable the body to behave as an object amongst objects in the world around, even in the absence of consciousness.

But without the phenomena of simultaneous and successive contrast these three factors, the power of endowing an event with reactive significance, physiological memory and projected characters, would be insufficient to account for the diversity of response yielded by the central nervous system. For, provided the stimulus remained unchanged both in strength and quality, an apparatus, working on purely mechanical principles, would continue to turn out identical products. This is notoriously not the case, even with the isolated spinal cord. A physically constant stimulus produces a steadily decreasing neural effect, and no two successive excitations, however similar, are followed by the same result. Contrast, simultaneous or successive, is one of the fundamental characteristics of vital processes in the central nervous system.

If we remain for a sufficient length of time in a room illuminated by red light, it will ultimately seem to be colourless. Adaptation has occurred, and the constant physical stimulus no longer produces its usual effect. But, when we emerge, everything seems to be tinged with green, an indication of the extent to which the neutral point has become displaced by physiological contrast during the action of the red light on the retina.

A constant physical stimulus cannot excite a constant vital effect within the central nervous system. At first, it is true, facilitation may occur; the reaction is obtained with greater ease and the field from which it is evoked is increased in extent. But soon the phenomena of reversal and deviation appear. The same stimulus, applied under apparently the same conditions, leads to an opposite result and may even inhibit the original reaction. Or some new feature appears, as when stimulation of a point on the cortex, previously associated with contraction of the face, suddenly yields movements of the elbow[1].

Such diverse and varying modes of response allow of close purposive adaptation, even on a physiological level; but they demand a high degree of vigilance in the structures responsible for the particular reaction. What wonder that a structural lesion of the brain, which affects speech, can disturb appreciation of the logical significance of internal or external events and destroy that habit-memory requisite for the perfect performance of symbolic formulation and expression.

6. PSYCHICAL AND SOMATIC BEHAVIOUR DEPENDENT ON THE STATE OF VIGILANCE OF THE NERVOUS SYSTEM.

Any one of the specific mental processes, which enter into the general stream of consciousness, may be eliminated without materially disturbing its other components. Provided the manifestations of the lesion are purely negative, any aspect of sensation, images, various forms of symbolic formulation, and even elementary states of ideation may be lost independently of one another. Sensation can be gravely affected, but images remain intact; the visual images of a patient, who has long suffered from hemianopia, do not differ from those of a normal man. Certain aspects of speech may be disturbed without any loss of general intelligence, except in as far as these particular acts are necessary for formal thinking. A lesion of the left pre-frontal region of the brain may affect the elementary processes underlying ideation, in such a way that it becomes possible to hold and to act upon two fundamentally incompatible ideas. One of my patients, who had received a small wound in the left frontal region, which injured the brain, appeared to be in every way normal. In daily intercourse he behaved rationally and showed executive ability in the work of the ward; but he wrote a long letter, asking detailed questions about the family, to his mother who had been dead for three years. He thought that there were two towns of Boulogne, one of which, on the homeward journey from the Front, lay near Newcastle; the other one, in France, was reached "after you had crossed the sea." This power of holding two mutually incompatible conceptions at the same moment lies at the basis of Korsakow's psychosis, so commonly the result of toxic agents, such as alcohol.

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[1] Sherrington, C. S., and Leyton, A. *Quart. Journ. of Exp. Physiol.* 1917, XI. 135, and several papers by Graham Brown in the same Journal, 1915, IX.

These various mental activities, sensation, imagery, symbolic formulation and ideation may be affected independently by an organic lesion, because the physiological processes, which subserve them, are bound up with the vitality of different parts of the brain, any one of which can be destroyed without of necessity affecting the others. Each such local disturbance is associated with some specific psychical loss of function. But the activity of the mind as a whole is uninterrupted; removal of one aptitude does not cause collapse of the whole structure, like a house of cards. The field of consciousness remains continuous as before; it closes over the gap as the sea leaves no trace of a rock that has crumbled away.

Perhaps these conceptions will be rendered clearer by the following simile. Intelligence of the movements of a hostile army can be gained from the air by the use of observation balloons, aeroplanes and dirigibles. Suppose that each form of aircraft depended for its existence on a separate aerodrome; any one class of machine could be eliminated independently, either by destroying its base or by reducing the number of skilled observers, pilots and mechanics below the level of efficiency. Now the sum of knowledge gained by observation from the air depends, not only on the messages communicated to the ground, but also on information signalled from one air-craft to another. Total disappearance of any one form of machine would diminish the sum of aerial knowledge without of necessity causing a break in continuity; but, on the other hand, the absence of this specific source of information could be recorded from the air as an observational fact.

As we pass in review from below upwards the various functions, which depend for their existence on the integrity of the central nervous system, sensation is the first to reveal the workings of consciousness; it forms the lowest mental level. But the afferent impulses, on which it depends, have already reached a stage of profound integration and, in the absence of the higher centres, are capable of initiating and controlling elaborate and purposive responses. Moreover, the anatomical centres, which govern sensation, are also responsible for a vast number of purely non-conscious and automatic functions. The same afferent impulses subserve sensation, maintain postural tone, and coordinate muscular movement.

Let us examine more closely the automatic activities of the central nervous system, to discover what relation they bear to those normally associated with some form of consciousness. I leave out of account altogether those mental states, termed sub-conscious, which have once formed part of the life of the mind and, although at the moment repressed or outside the focus of attention, may under favourable conditions exert some direct psychical influence. I am concerned solely with those processes, which can manifest themselves, when the highest cerebral centres are out of action.

Many of these responses, as I have already indicated, show a high degree of purposive adaptation. The various integrative processes, which have occurred, insure reaction of the body as a whole; a reflex in a decerebrate cat may extend from its ears to the tip of the tail. This direction of behaviour towards a definite end is brought about by three factors.

Firstly, the central nervous system exercises selection amongst the diverse and massive physical influences acting on the organism from without; some of these are effective in arousing a neural reply, others pass unnoticed. Each adequate stimulus is followed by an appropriate response, which becomes increasingly specific with each rise in functional level. But if any two are applied together, the reaction which follows does not of necessity express their algebraic summation; more often one is favoured at the expense of the other. Qualitative selection has occurred in the absence of consciousness, and external objects have acquired a reactive significance.

Secondly, the response at any one moment is not an isolated phenomenon, but forms part of a march of events in which the past exerts a profound determining influence; at the same time the future is implicit in the present. This insures that the activities of the central nervous system shall possess duration, the basis of physiological memory.

Lastly, the spacial elements in afferent impulses endow even non-conscious functions with projection. The responses of a decerebrate animal are directed, not only to a certain end, but also to a definite place. Its behaviour is governed both by relations of space and time.

Sensation, by endowing us with awareness of these reactions, contains the elements of consciousness. As we ascend further step by step in the nervous system, each specific nodal point of vital activity is associated with some more highly developed psychical aptitude and the mental functions increase in complexity with each rise in physiological level.

But, so long as we confine our attention to those specific aspects of psychical defect, which follow lesions of the higher anatomical centres, we find no function that cannot be discovered in a primitive form amongst the reactions of the isolated nervous system. Selective responses give meaning and purpose to external and internal events. Memory and intention are developed out of that march or sequence of occurrences, which constitutes duration. A succession of momentary happenings would have no continuity; a series of perishing nows would be useless, even at the lowest level of neural activity. Finally, the power of spacial projection enables us to endow external objects in the world around with those qualitative attributes, which are developed out of selective responses to physical stimulation. Our perceptual knowledge is the finding of ourselves in events.

Consciousness stands in the same relation to the vigilance of the higher centres as adapted and purposive reflexes to that of those of lower rank in the neural hierarchy. When vigilance is high, mind and body are poised in readiness to respond to any event external or internal. But if it is lowered by injury, want of nutrition, chloroform or any other toxic influence, these high-grade functions may suffer in general or in part, whether they are associated with consciousness or not.

There is no absolute criterion by which an external observer can distinguish conscious from unconscious behaviour. A man may respond, in an apparently normal manner by act or cry to a painful stimulus, when insensible from chloroform, and can babble intelligibly during delirium or sleep.

Mind and body habitually respond together to external or internal events. The aim of the evolutionary development of the central nervous system is to integrate its diverse and contradictory reactions, so as to produce a coherent result, adapted to the welfare of the organism as a whole. This unification is the product of its vital activity from the lowest spinal to the highest cerebral centres. There is no more difficulty in understanding how an act of consciousness can affect a physiological process, than to comprehend how one reflex can control and modify another of a lower order.

Consider, for example, the conditions, which govern micturition. When the fluid in the bladder reaches a certain variable amount, the muscular wall contracts, the sphincter relaxes and the contents are expelled. After transection of the spinal cord, this whole process may occur automatically.

Should the cord reach a high state of vigilance, the act can be greatly facilitated by scratching the sole of the foot or other parts of the body below the lesion, and the bladder is emptied at a smaller volume. But, as soon as the controlling influence of the mid-brain can be exerted upon the spinal centres, this facilitation ceases; the stimulus to be effective must be applied within the limited field of the bladder itself.

Psychical conditions have a profound effect upon this reflex. It may become urgent and uncontrollable under the influence of fear; animals evacuate the bladder preparatory to flight, and the language of schoolboys has preserved this association between states of emotion and the passage of urine. On the contrary, it can be checked by an idea; all desire to micturate dies away with the knowledge that place and time are unsuitable for performance of the act.

Here mind and body are intimately associated in facilitation and control of one of the lowest reflex actions of the spinal cord. We can recognise by introspection that, at any one moment, consciousness played a part or was absent from the sequence of events; but, whatever the level, conscious or automatic, on which this control is exercised, it would have been impossible without a high degree of vigilance. Lowered vitality in the spinal, or mid-brain, or cerebral centres governing micturition will be associated with some variety of incontinence or retention of urine. The common factor in psychical and physiological processes is vigilance; to express this relation and avoid the words 'conscious' and 'unconscious,' we might speak of 'psychical' and 'somatic reactions.'

This is even more evident, when we consider the manifestations of a high intellectual aptitude, such as speech. It is acquired during the lifetime of the individual, not as an isolated faculty, but as a sequence of psychical and physiological processes. It is a mode of behaviour in which mind and body are inseparable. The one essential and obligatory condition is a state of vigilance in the cerebral centres that participate in the act.

Perfect symbolic formulation and expression demands the capacity to manipulate freely and at will images, meanings, intentions, feelings and ideas, for a specific purpose. In aphasia and kindred disorders of speech all these psychical functions are more or less affected, in as far as they participate in this mode of behaviour; apart from it, they may remain entirely undisturbed. A specific form of psychical activity and its subservient physiological processes have been thrown into disorder, but general consciousness and the mind as a whole remain intact.

The total sum of the activities, which we call the mind, results from the interaction of all these psychical and somatic reactions. For just as a purely automatic response shows obvious evidence of integration, so the various functions associated with consciousness influence one another profoundly. But, although any one of them can be lost independently, they cannot be said to be the 'elements' out of which general consciousness is 'composed'; they form rather forces in a psychical interplay. Any one of them may be eliminated without disturbing the continuity or coherence of the mind. The aim of consciousness is to produce a unity of action directed to the welfare of the organism as a whole.

The mind is said to be aware of its own activities and to be conscious of itself as an object amongst objects. But vigilant behaviour, even on purely automatic levels, is directed in accordance with factors of time and space; somatic reflexes, of a sufficiently high order, cause the body to assume attitudes adapted in purpose to external events.

The main reason why the processes of the mind seem not only to transcend, but to differ in nature from these automatisms, lies in the enormous development of the power of projection at the ideational level. Even sensation can be projected to the end of a stick or tool held in the hand; with an expert shot, the gun is an active member of his body and the pace with which it swings is an exactly regulated somatic function. In speech this power is raised to an even higher degree; for here the act is directed not only towards an external object, but to the intelligence of another person. In the processes of ideation the capacity for projection reaches vast proportions; the astronomer can move as securely among the stars as most of us between the walls of a familiar room.

The mind is the resultant of a multitude of forces, which culminate in both conscious and unconscious activities. Once generated, these interact, producing a continuum in space and time. But analysis of the behaviour they produce shows that, from the highest to the lowest, they exhibit certain characters in common, which depend on qualitative selection, duration and projection. These cannot exist without a high degree of vigilance, in those parts of the central nervous system responsible for the reaction. Any influence, which lowers neural potency, acting on some appropriate portion of the central nervous system, even for a moment, may abolish consciousness as a whole or eliminate some specific group of psychical or somatic responses. Should the disturbing agent exert a universal effect, as is the case with chloroform, one function after another disappears in order, beginning with those of highest rank and culminating with the most mechanical and pre-ordained responses.

7. SUMMARY.

1. The central nervous system is capable of reacting as a whole or in part with different degrees of physiological efficiency, which varies with its vital condition. When this is raised, a response to stimulation is not only obtained more easily, but shows signs of belonging to a higher order than when it is low.

2. The extent to which the activities of a particular portion of the central nervous system exhibit at any moment signs of integration and purposive adaptation indicate its vigilance. When vigilance is high, the body is more prepared to respond to an effective stimulus with a more or less appropriate reaction.

3. Vigilance is diminished, not only by structural changes in the central nervous system, but also by toxic influences such as chloroform, sepsis, or anything which tends to lower physiological capacity.

4. This is profoundly evident in all automatic actions, even those which have been acquired during the life of the individual. These aptitudes rise and fall according to the vigilance of the parts responsible for their execution.

5. In all specific modes of behaviour, conscious and automatic processes are inextricably mingled. Normal sensation demands not only recognition of qualitative and spacial differences, but also accurate registration on a physiological level of the results produced by previous afferent impressions.

For example, the same class of impulses which determines the realisation of spacial relationships is responsible for purely physiological conditions, such as the maintenance of some forms of muscular tone and coordination. Normally mind and body must be prepared to respond immediately with an appropriate reaction.

6. Purposive adaptation is more or less evident in all responses, somatic or psychical, which occur in a high state of neural vigilance.

Three factors are responsible for this character in the response. Firstly, certain qualities in the stimulating object are ignored, whilst others give rise to a reaction; these reactions in turn struggle among themselves for mastery and their final integration gives significance to one aspect of the stimulus rather than another. Secondly, the form assumed by the response at any one moment depends on dispositions due to past activities, and the future is implicit in the present. Thus the behaviour of the central nervous system, in whole or in part, becomes an orderly march of events and is not a series of isolated episodes. Thirdly, afferent impulses endow the response with spacial relations; the resultant action is coordinated to a definite end in space and time.

7. The same factors in a more developed form can be discovered in all conscious modes of behaviour, which are affected specifically by disorders of the central nervous system. Reactive significance endows the psychical act with meaning; duration forms the basis of memory and intention; whilst the power of projection enables us to attribute to external or internal events those properties we have selected from amongst the impressions they have left upon us.

8. Conscious processes bear the same relation to the life of the higher centres of the nervous system as purposive reflexes to the vitality of those lower in the neural hierarchy. All alike are the expression of physiological vigilance.

9. The unity of the mind is the resultant of all those psychical processes, which can in any way modify consciousness. These interact and profoundly influence one another, to produce in the end a continuum of highly purposive adaptations. This is not the result of a synthesis of diverse elements, but is the final consequence of the struggle amongst various forms of response. Should any one of them be eliminated, there is no gap in the continuity of consciousness; the loss of function can be recognised solely by a definite act of observation.

THE CONCEPTION OF NERVOUS AND MENTAL ENERGY[1] (III).

By C. S. MYERS.

I have only at the last moment been induced to join in this Symposium, because of the unexpected inability of a French and of an American psychologist to contribute to it, and owing to a certain incompleteness in which the treatment of the subject has consequently been left. I cannot hope to do much more than fill in a few gaps and to indicate additional problems for discussion.

In his admirable paper Dr Adrian has directed our attention to the experimental evidence (largely his own) that the activity of medullated nerve fibres manifests itself as a rapid succession of impulses, each of which is followed by a refractory period—a period of recovery—during which a fresh impulse cannot be developed. The corresponding activity of striated muscle fibres manifests itself as a fusion of successive twitches, and after extreme activity these fibres also require a period of recovery; but whether these periods in muscle and nerve are at all analogous is reasonably open to doubt. However, there is evidence (also partly due to Dr Adrian's investigations) that striated muscle fibres and medullated nerve fibres agree in reacting to stimuli according to the 'all or none' principle. That is to say, in any fibre there is no gradation of reaction according to stimulus strength—although individual fibres may differ *inter se* as to sensitivity to a given stimulus strength. So too, in the heat and cold spots of the skin (according to Dr Head) the reaction is largely ungraded—'all or none,' and time is required for recovery after each such momentary reaction.

In the case of peripheral nerves, all that we know of the nature of their activity is an electric response; and this must be produced by a movement of ions in the active region of the nerve fibre. In the case of striated muscle fibres we also recognize definite physical (including chemical) changes. Dr Adrian holds that because nervous (and muscular) activity thus reveals itself to us as "a special manifestation of physical energy...transferable into other forms of energy in accordance with the laws of thermodynamics," the conception of a special (peripheral) nervous (and muscular) energy is unnecessary. But, as I shall immediately indicate, there is another form of muscular and nervous activity, of the physical nature of which we know almost nothing; moreover, there is the activity of the central and autonomic nervous systems, of the physical nature of which we know even less. What we do know about these activities is that they are capable of affecting muscular activity and hence presumably physical forms of energy. Differing here from Dr Adrian, I maintain that so long as we recognize that living tissue displays activity which can ultimately manifest itself as increase or decrease of physical work, no harm can result from applying the term 'energy,' even though we are ignorant of its nature and are unable directly to measure it in terms of mass and velocity.

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[1] Read as part of a Symposium at the Oxford Meeting of the 7th International Congress of Psychology, 30 July 1923.

What I wish to suggest is that there are two different systems governing nervous and muscular activity—the one concerned in the development of muscular contractions, ungraded and susceptible of 'exhaustion' (to this we have already alluded), the other concerned in the development of muscular posture and tone, graded and susceptible of 'adaptation.' We carry our head erect and our lower jaw raised for many, many hours 'on end,' without manifesting or experiencing fatigue. Our muscles are perpetually kept in a state of (varying) tone—*i.e.* tonic contraction—throughout life. So too we can unchangingly endure the continuous light of a northern summer. In this second system, the phenomena of adaptation (and of contrast) replace those of fatigue; sometimes, *e.g.* on prolonged stimulation by warmth, coolness, or colours, where the original sensation disappears with adaptation, the two systems seem somehow to be combined.

For this manifestation muscular posture and tone apparently demand an intact central nervous system and the cooperation of afferent impulses from antagonistic muscles. May it not be that the instances of thermal and visual adaptation, just mentioned, also depend on a similar integration of two sets of antagonistic apparatus—for light and dark, red and green, warm and cool, etc.? If so, are we not forced to conceive of a second kind of activity, totally different from that stressed by Dr Adrian in his contribution to this Symposium? Perhaps it may consist in *a certain molecular 'set'* of the muscular, peripheral nervous and receptor living substance, by virtue of which the phenomena of prolonged adaptation (and contrast effects) replace those that depend on *the consumption of available material* manufactured by the cell to provide intermittent flashes of activity. This 'set' seems to involve a temporary static change (or 'attitude') in the living substance, and to be produced and maintained by prolonged, not too intense, stimulation, that is to say, by a fairly uniform environment. Whether the ultimate *effect* of this 'set' be to increase or to decrease expenditure of energy, energy is certainly required in order to *maintain* the 'set'; and we may, I think, most reasonably assume that what maintains it is the activity of the nerve cell, by virtue of its special, 'higher' connexions and combination with other nerve cells concerned in antagonistic motor functions, thus permitting of reciprocal inhibition and facilitation. Anyhow, energy has to be employed, work has to be done in maintaining tone, posture, inhibition or attitude; and ultimately even here fatigue sets in.

Is it too bold to attempt to recognize these two forms of fatigue in intellectual activity—the one 'explosive,' intermittent, arising from the need to make numerous judgments and decisions, and the other 'adaptative,' continuous, arising from the need to preserve a certain mental 'set' or 'attitude'? This attitude is at first 'spontaneously' maintained by the energy derived from 'interest.' But as the latter tires, conscious voluntary efforts (of the first variety), have to be made to uphold the attentive attitude; the feeling of interest gives place to that of boredom; until finally mental weariness and even mild mental 'disorder' arise.

But it must not be supposed that conscious volitional activity always involves the intermittent 'flow' while unconscious activity involves the continuous 'flow' of energy. All our present knowledge favours the view that, whether (so far as we can ascertain) consciousness is present or absent, no difference need occur in the result. Moreover, as Dr Head has so convincingly shown, certain marks of conscious activity are to be found even at the lowest levels of the central nervous system. Probably in all forms of living matter both forms of activity in varying degree co-exist.

Yet it is difficult not to regard the highest levels of mental activity as predominantly constructive and directive, acting on lower, *relatively* mechanical, systems, much as Clerk Maxwell's hypothetical demons worked in sorting out gaseous molecules or as a train may 'purposefully' make its way now along one set of lines, now along another, now along a third, by external direction. But however much it may differ from blind mechanical activity, such external direction must surely involve work; we seem bound to suppose that, in giving effect to it, energy—alike in the psychological and in the physiological aspect and hence ultimately in the physical—is expended. Work must be done in switching the rail points, in setting the neural paths, in modifying the neural 'centres,' now in one, now in another direction—leading to inhibition, dissociation, regression, or their opposites. Whether or not there be a common fund of '*intellective*' energy (for such it is at the highest levels, although we recognize it, to use Dr Head's significant term, as 'vigilance' at the lowest, 'physiological' levels) which may be employed in the various intellectual abilities with which different minds are differently endowed, is a problem still awaiting solution.

To-day, however, the swing of the pendulum tends to ascribe all mental activity to 'feeling,' and to suppose that all percepts, ideas and volitions, all forms of cognition and conation, derive their motor effects from the energy which they obtain from related affects. According to some, indeed, this energy is ultimately to be derived from a single affect,—the sexual emotion. But the past neglect of instinctive and emotional feelings should not cause us to overlook the activity involved in perceiving or thinking, or to regard percepts or thoughts (*e.g.* ends) as merely inert 'mental matter' whose 'movement' (nay, whose very 'existence' in consciousness) is dependent *solely* on the force of propulsion or repression derived from feeling. Cognitive and affective experiences are not thus to be isolated in their beginnings. Moreover, as regards volition, self-activity is not identifiable with instinctive and emotional activity: the activity of the 'individual' organism is not the mere sum or resultant of the activity of its abstract parts.

The last point to which I would invite attention is the relation between mental and nervous energy. Energy in general we recognise only by the work of which it is capable; we actually measure it only by the work which it performs; and this work involves a transformation of energy from a higher to a lower level. We know nothing of what, *e.g.* electrical energy really is: we describe it in lower terms, *e.g.* of inertia, mass, weight and structure, with which, indeed, according to most recent theory, all energy is endowed. At the same time, however, we do not identify the various forms of energy with each other. Should we, then, because peripheral nervous energy reveals itself as electrical energy, necessarily follow Dr Adrian in identifying peripheral nervous with electrical energy? The latter may surely be a 'degradation' of the former. Still more cogent, to my mind, becomes the acceptance of a special nervous energy when we come to deal with the central nervous system. In our present ignorance I see nothing in such acceptance necessarily incompatible with the doctrine of the conservation of energy.

I feel convinced that the problems of inhibition, control, direction and purpose which we have to face on the psychological side cannot honestly be evaded on the physiological side—in other words, that the outstanding problems of mind and life must ultimately be solved in similar terms.

I see no reason why we should not throughout identify central nervous energy with mental energy—Dr Head's 'physiological vigilance' with the psychologist's 'mental activity.' I find no difficulty in supposing that this 'psycho-neural' energy, as I would term central nervous energy, assumes a more distinctively mental form as higher, wider and more plastic nervous areas are brought into function.

The only consciousness about which we know anything is self-experience. It is here that we should expect the highest expression of what I have termed 'psycho-neural energy.' Self-consciousness becomes fundamentally the activity of a hierarchy of the highest nervous elements that are concerned in the most important past and present (? and future) responses of the organism to its environment which inter-relatedly are susceptible of control. Examining the three modes of consciousness we find that the conative disappears when practice and habit no longer necessitate control and direction by that integration of psycho-neural activities which we call 'the self'; that the affective mode also always implies conflict with and inhibition of antagonistic reactions; and that the cognitive mode likewise involves alternative reactions; were the response of the individual to a given stimulus fixed, nothing would be served in his becoming cognitively conscious of it.

A STUDY OF VISUAL AFTER-SENSATIONS WITH SPECIAL REFERENCE TO ILLUMINATION IN COAL MINES

By E. FARMER, S. ADAMS AND A. STEPHENSON.

(*A Report to the National Institute of Industrial Psychology.*)

I. *Object of the experiments* (pp. 153–154).
II. *Description of the experiments and tabulation of data* (pp. 154–157).
III. *Results of these experiments* (pp. 157–159).
IV. *Relation of visual acuity and physical intensity of stimulus* (pp. 159–162).
V. *Experiments on nystagmic miners* (p. 162).
VI. *Conclusions* (pp. 162–163).

I. OBJECT OF THE EXPERIMENTS.

DURING a prolonged investigation in a coal mine we became convinced that the efficiency of the miner's lamp played a considerable part in his mental and physical well-being; and in order to test this conviction we decided to carry out a series of experiments in the laboratory. As in the course of our work in the pit we found that the small localised points of light emitted by the miners' lamps produced in us persistent and annoying after-sensations, we determined to ascertain whether the miners themselves suffered similarly, and if so to devise some method of mitigating the evil.

It may be objected that strangers like ourselves visiting various parts of the pit would naturally come in contact with the direct stimulation of various lamps, but that miners working on the coal face would not be subjected to the same amount of stimulation.

To some extent this is true, but it is by no means the whole truth. The miners place their lamps in such a position that the light is thrown on the coal face. This is quite satisfactory so long as the miner is picking coal or dirt on the face itself. Picking, however, is only one of his many activities; he also shovels coal into 'tubs' which are behind him, and dirt into the 'pack' which is likewise behind him. In the performance of these duties the position of the lamp, which was satisfactory while he was working on the coal face, is no longer so; and in shovelling, his movements cause him to receive the light directly into his eyes as frequently as forty times per minute. Furthermore, it must be borne in mind that, as a general rule, other members of a group of miners place their lamps in positions which, though suitable for themselves, may be extremely inconvenient for their neighbours. Another occasion when miners are subjected to stimulation by the lamps of other miners is when they are travelling in single file to or from the coal face. Indeed it was in this connexion that we first found that the miners were aware that a momentary glance at a lamp produced a feeling of discomfort in the eyes.

II. DESCRIPTION OF THE EXPERIMENTS AND TABULATION OF DATA.

The experiments were carried out in the dark-room of the Psychological Laboratory at Manchester University which was kindly lent to us for the purpose by Professor Pear.

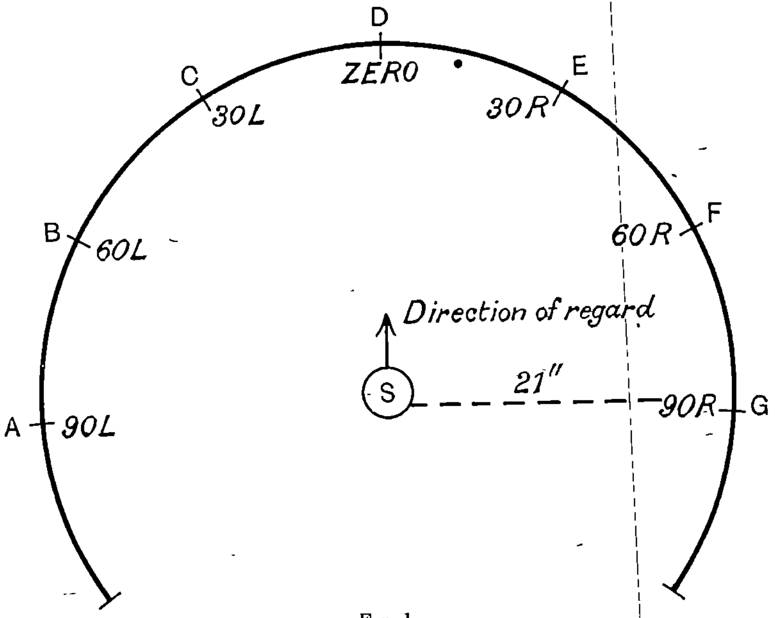

Fig. 1.

Pit conditions were reproduced as far as possible, a standard electric lamp of one foot-candle power being used throughout the experiments. As it was necessary to insure the same period of stimulation in all cases, a kymograph drum was fitted with small brass strips of such lengths that contact with the respective strips while the drum was rotating produced a stimulus lasting 1, 2, 3, 4 or 5 seconds. By experimenting on each other we found that a stimulus of two seconds' duration sufficed to produce after-sensations which persisted long enough to be described, and recurred often enough to be counted.

The subject was invariably seated at the centre of an arc of a circle of 21 inches' radius, his head supported by a chin rest; and he was instructed to fixate a small white spot at eye level on the black wall opposite. The stimulus was applied by switching on the light at one of seven positions on the arc, viz. a zero position directly in front of the subject, 30, 60, or 90 degrees to his right or left (cf. Fig. 1). The stimulus was applied for two seconds, the subject being instructed to look straight ahead after the extinction of the light and to describe what he experienced. One experimenter controlled the switching on and off and the alteration of the lamp position, while the other started a stop watch as soon as the light was extinguished. The subject was asked to count the number of after-sensations and also to say when they ceased. The results of these introspections were noted by the experimenter and are given in the following tables. The complete series of seven positions was repeated five times with each subject so that the results given are based on five readings for each position.

The experiment was carried out on four non-mining subjects and also on five mining subjects and the results are given separately.

Table I is a typical example of a miner's results with a 2-seconds' stimulation and the standard lamp at eye level. It is given separately in order to illustrate the method employed.

Table I.

In all tables 'No' refers to the number of recurrences of image and 'D' to the duration of the after-effects in seconds.

	90° L		60° L		30° L		Zero		30° R		60° R		90° R	
	No	D	No	D	No	D	No	D	No	D	No	D	No	D
	1	15	1	12	1	14	6	67	1	12	1	15	1	10
	1	17	1	19	1	20	6	52	1	12	1	16	—	—
	1	20	1	20	1	14	6	61	1	14	1	17	1	15
	1	18	1	21	2	12	8	52	1	13	1	15	—	—
	1	10	1	20	2	26	8	45	1	14	1	19	—	—
Average	1	16	1	18·4	1·4	17·2	6·8	55·4	1	13	1	16·4	0·4	5

Table II gives the results obtained from the four non-mining subjects with a 2-seconds' stimulation and the standard lamp at eye level.

Table II.

	90° L		60° L		30° L		Zero		30° R		60° R		90° R	
	No	D	No	D	No	D	No	D	No	D	No	D	No	D
B. H.	1	38 6	1	34·2	1	35·6	1 2	80 2	1	31·6	1	40 4	0·4	14·4
S. A.	1	35·2	1 4	40·2	1·4	35·4	8·8	113·6	1 2	30 6	1·2	36·2	0 8	25
S. P.	1	21·2	1	26·2	1·4	27·4	1 6	59·6	1·6	23·8	1	30·6	1	22·8
A. S.	1	18	1	22	1·6	23 6	2 4	55·4	1·6	27·6	1	22·2	1	22·6

Table III gives the results obtained from the five mining subjects with a 2-seconds' stimulation and the standard lamp at eye level.

Table III.

	90° L		60° L		30° L		Zero		30° R		60° R		90° R	
	No	D	No	D	No	D	No	D	No	D	No	D	No	D
J O'N.	0 6	14·4	0·8	23 2	1	31·6	1	33·4	1	29	1	36 8	1	31·4
B. O'N.	1	18·2	1	16·2	1	15·2	1·6	24 4	1	14	1	12 8	1	16·2
S. L.	1	19·2	1	23 8	1·2	31	1	61 2	1	23·8	1	22·8	1 2	25·6
G. F.	0·6	12·8	2	35·6	3 2	33 2	4·2	29	5·2	37 6	1·6	30·8	1·2	23·4
D. R.	1	16	1	18 4	1·4	17·2	6·8	55 4	1	13	1	16·4	0·4	5

Twenty-seven miners were afterwards tested with the standard lamp at the zero position only, in order to ascertain how many of them were subject to after-sensations. These results, averaged from five readings, are given in Table IV.

Table IV.

Subject	No	D	Subject	No	D	Subject	No	D	Subject	No	D
1	1	13	8	0 8	10·8	15	*	99 8	22	1·4	51·6
2	1	16·4	9	2 2	55·4	16	2 4	48·8	23	0·2	7·2
3	8 4	105	10	1·8	47	17	1	23	24	1	32
4	0·8	21	11	1 6	33·8	18	5·6	66·8	25	*	111·6
5	0·4	45·4	12	1·2	47·8	19	*	59 6	26	1	20·4
6	0·6	22	13	*	114	20	7	79	27	7	77·4
7	3	59·2	14	*	91 8	21	1	26·2			

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* In these instances the subjects experienced so many after-sensations that they were unable to record the number.

Two subjects were tested with the standard lamp one foot above eye level and one foot below eye level. The results, averaged from five readings, are given in Table V.

Table V

Subject S. A.

	90° L		60° L		30° L		Zero		30° R		60° R		90° R	
	No	D	No	D	No	D	No	D	No	D	No	D	No	D
Eye level	1	35·2	1·4	40·2	1·4	35·4	8·8	113·6	1·2	30·6	1·2	36·2	0·8	25
1′ above eye level	—	—	0·8	20·8	1·4	28·8	0·8	19·2	0·6	16·2	0·8	20·6	—	—
1′ below eye level	0·2	4·6	1	25·6	1	24·4	1·2	39·8	1	26·6	1	27	1	22
Subject A. S.														
Eye level	1	18	1	22	1·6	23·6	2·4	55·4	1·6	27·6	1	22·2	1	22 6
1′ above eye level	—	—	0·6	12·4	1	22·2	0·8	41·2	1·4	30·8	0·6	16·4	—	—
1′ below eye level	1	16	1	17·6	1	18·4	1·6	34·2	1·2	23·4	1	16	1	22·8

Experiments were also carried out on two subjects with the bulb and the outer glass of the lamp treated with various preparations calculated to soften the effect of the light stimulation and also to alter its colour. The results are given in Table VI.

Table VI.

Bulb	Outer glass	90° L		60° L		30° L		Zero		30° R		60° R		90° R	
		No	D	No	D		D	No	D	No	D	No	D	No	D
Ordinary	Ordinary	1	18	1	22	1·6	23·6	2·4	55·4	1·6	27·6	1	22·2	1	22·6
Ordinary	Painted inside and outside with hydrofluoric acid	1	18·6	1	22·4	1·2	19·2	1	20·2	1	19·4	1	21·8	1	21·4
Ordinary	Slip of green gelatine inserted	1	19	1	19·6	1	18·4	1	27·8	1	20·2	1	18	1	19·8
Ordinary	Painted inside with hydrofluoric acid	1	21·8	1	24	1	23	1	24	1	21·8	1	25·2	1	22·6
Painted with hydrofluoric acid	Ordinary	1	20·8	1	25·6	1	20·6	1·2	24	1	23·8	1	24 6	1	23·8
Painted with hydrofluoric acid	Green gelatine slip inserted	1	19·6	1	22 6	1	22	1	18	1	23·8	1	22·6	1	14·4

III. RESULTS OF THESE EXPERIMENTS.

The zero position at eye level, when a lamp with ordinary bulb and ordinary outer protecting glass was used, results in the greatest number and duration of after-sensations. They are somewhat reduced when the lamp is placed one foot above or one foot below eye level.

Peripheral stimulation appears to be generally less effective when the lamp is one foot above eye level than when the lamp is one foot below eye level.

Peripheral stimulation even at 90 degrees to the right or left of the subject, with lamp at eye level, yields after-sensations of considerable duration.

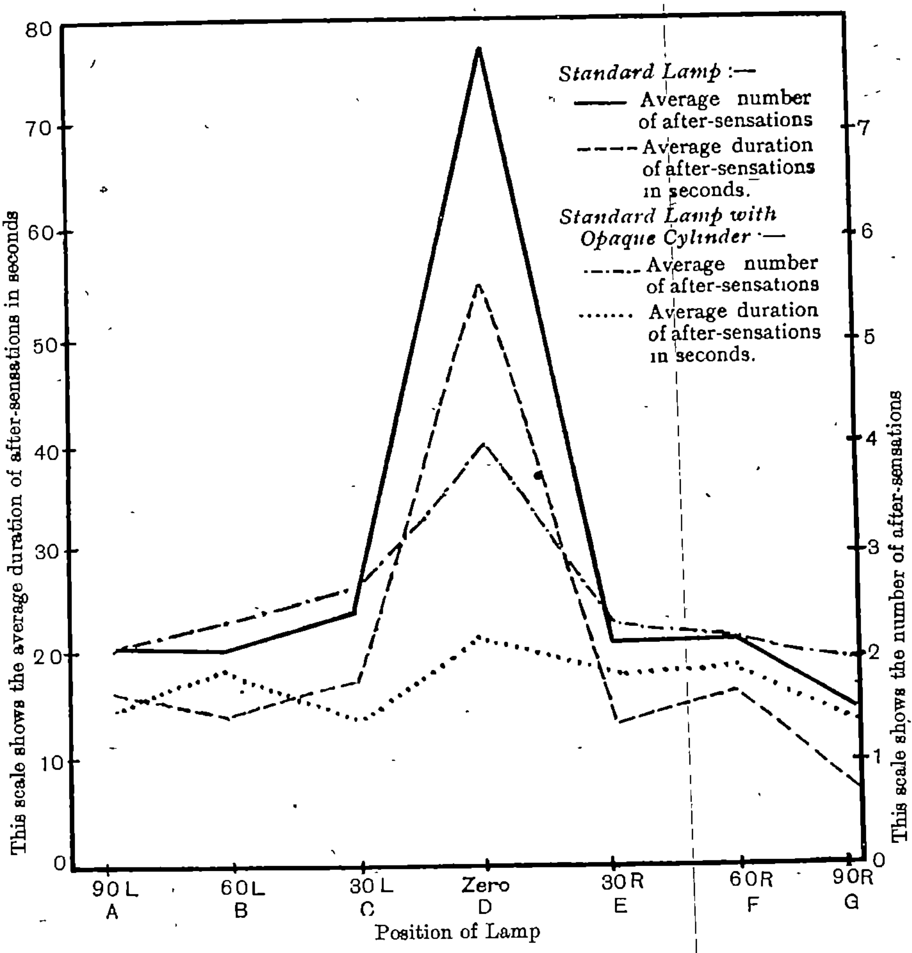

Diagram I.

Of a total of 37 subjects who could appreciate the presence of after-images, 35 reported considerable movement of the image when the ordinary lamp was employed. One miner, abandoning the chin rest, actually turned through 180 degrees to follow the image, although he had been instructed to sit still and to look straight ahead.

With the ordinary lamp the reflexions of the incandescent filament in the concave glass of the bulb and in the outer protecting glass were reproduced as images, most markedly in the zero position. Subjects reported two crescents, three crescents, snakes, wires, star with scatter, pulsating rings and crescents, contracting circles, etc.

Practically every device which overcame the sharp localisation of the light of the ordinary lamp reduced the movement of the image, and also the number and duration of the after-sensations (cf. Diagram I).

The colour effects due to the gelatines used did not meet with the approval of the miners.

The outer glass, when rendered translucent, reduced considerably the contrast between light and shadow which is most marked in the case of the ordinary lamp. Furthermore the harsh effect of the small point of light in the case of the ordinary filament was softened.

The simplest device to secure translucence, for practical purposes, and one which met with approval from the miners, was that of painting the inside of the outer protecting glass with hydrofluoric acid, the outside being left smooth so that it did not collect coal dust unduly.

A dozen lamps were so treated and used by miners over a period of six weeks. The manager's report is here given *verbatim*.—"The men who have worked with the electric lamps fitted with opaque protecting glasses are very pleased with them, finding their work more comfortable, less strain on their eyes, and would not like to go back to the ordinary clear glasses again."

Numerous requests for these glasses were received from other miners, and some hundreds have now been supplied. The following are remarks made by users on the coal face.—"It's better for the eyes." "I like the new glasses to be also in the places on each side; my eyes are not bothered then." "It's aw reet—it doesn't make me blink" (nystagmus). "It's a good un—if I didn't like it I should tell thee." "It doesn't cast any shadows." "Since I've used this new glass my eyes have not bothered me when I've got up the pit after work."

IV. RELATION OF VISUAL ACUITY AND PHYSICAL INTENSITY OF STIMULUS.

A further investigation was carried out on thirty miners in order to determine whether the lowering of physical intensity caused by painting the outside glass of the miner's lamp with hydrofluoric acid affected visual acuity. In order to test visual acuity the figure described by J. C. Flügel[1] was used. It consisted of a circle of radius ·5 cm. with a square, the sides of which were ·5 cm., on the circumference. This was illuminated by a miner's electric lamp, 'standard' or 'translucent,' which was hung on a pit prop at a distance of one yard from the figure. It will be seen from the drawing (Fig. 2) that the pit prop also served to shield the subject's eyes from the direct light of the lamp. The position of the square was manipulated from the back, the figure being mounted on the head of a nail. The subject was required to describe the position of the square by regarding it as the hour hand of a clock. To ensure that the subject understood the method of reading he was given preliminary practice with a large demonstration figure. The subject was then placed at a distance of six metres from the figure which was brought nearer to him until a stage was reached when he read correctly three positions of the square out of a possible three. If the subject succeeded at six metres with the large figure, a smaller figure (radius = side of square = ·34 cm.) was substituted, and the same procedure was repeated.

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[1] This *Journal*, XI. 291.

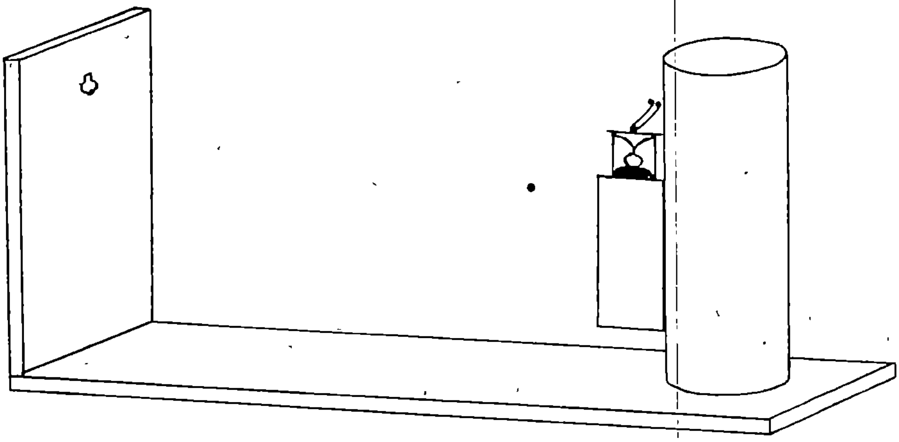

Fig. 2.

The results are given in Table VII.

In fifteen persons the visual acuity was better when the lamp provided with a translucent cylinder was used than with the standard lamp. In six of these cases the lamp with the translucent cylinder was employed first and in nine cases the standard lamp was employed first.

In nine persons the visual acuity was equal in the case of both lamps. In four of these cases the standard lamp was used first, and in five cases the lamp with translucent cylinder was used first.

In four persons the visual acuity was better with the standard lamp than with the lamp with translucent cylinder. In two of these cases the standard lamp was presented first, and in two cases the lamp with translucent cylinder was presented first.

Table VII.

	Type of lamp					
	'Standard'		'Translucent'			
	Test-figure		Test-figure			Order of presentation†
Subject	Large	Small	Large	Small	Acuities compared*	
W. B.	4·5	.	4·5	.	=	S
W. A.	.	5	.	6	+	T
X. B.	5·5	.	6	.	+	S
X. A.	5·5	.	5	.	−	T
W. D.	5	.	5·5	.	+	S
W. C.	2·5	.	3·5	.	+	T
X. D.	4	.	5·5	.	+	S
X. C.	5	.	5	.	=	T
W. F.	5	.	4·5	.	−	S
W. E.	5	.	4	.	−	T
X. F.	4	.	5	.	+	S
X. E.	5·5	.	6	.	+	T
W. H.	3·5	.	3·5	.	=	S
W. G.	5·5	.	.	4	+	T
X. H.	.	5	.	6	+	S
X G.	Fail	.	Fail	.	.	T
X. J.	.	4·5	.	5	+	S
X. I.	4·5	.	4·5	.	=	T
W. J.	3·5	.	4	.	+	S
W. I.	4	.	4	.	=	T
X. L.	.	6	.	6	=	S
X. K.	5·5	.	5·5	.	=	T
W. L.	4·5	.	4·5	.	=	S
W. K.	Fail	.	2·5	.	+	T
X. N.	.	4	.	4·5	+	S
X. M.	4·5	.	4·5	.	=	T
W. N.	.	5·5	.	4·5	−	S
W. M.	5·5	.	.	4·5	+	T
W. O.	.	5	.	5·5	+	S
X. O.	Fail	.	Fail	.	.	T

* Where better visual acuity results from using the lamp with translucent cylinder (frosted outer glass) the fact is indicated by + in this column. Where visual acuity is equal for both types of lamp the fact is shown by = in this column. Where visual acuity is better with the standard lamp the fact is indicated by − in this column.

† S or T in this column indicates that the standard lamp, or the lamp with translucent cylinder respectively, was presented *first*.

The comparative intensities of the 'standard' and 'translucent' lamps were measured with a Holophane Lumeter by two independent observers, who each took ten readings with each lamp. It was found that when the outer glass of the lamp was painted with hydrofluoric acid a 28 per cent. reduction in physical intensity resulted.

Despite this 28 per cent. reduction in intensity, the visual acuity of 80 per cent. of the 30 persons tested was, as we have seen, improved or at all events not diminished by the substitution of a diffused light.

V. EXPERIMENTS ON NYSTAGMIC MINERS.

It seems clear from the foregoing experiments that the present type of lamp used in coal mines causes considerable discomfort to the miner and also that the position in which he places it is of great importance. The nystagmic subjects who were experimented upon reported similar after-sensations, but the number and duration of the latter was greater than in the case of the non-nystagmic subjects. One nystagmic subject, after being stimulated as usual by the ordinary lamp, described his after-sensations by saying "the wires are almost like the real thing." When stimulated by a lamp the outer glass of which had been painted with hydrofluoric acid, he said "the movement is very slight."

It is probable that the after-images caused by the direct stimulation of the miners' lamp constitute one of the contributing causes of miners' nystagmus. The conclusion arrived at by Rivers[1] that miners' nystagmus is in part of psycho-neurotic origin would tend to confirm this suggestion. A miner suffering from some mental disturbance is likely to be more sensitive to the irritating effect of after-sensations, and to pay undue attention to them; in this way he would be helping the development of psycho-neurotic symptoms, and hence increasing the severity of the nystagmus. If the effect of the after-sensations can be lessened, the degree of nystagmus would probably be lessened with removal of one of the factors favourable to psycho-neurotic disturbance.

VI. CONCLUSIONS.

The number and the duration of visual after-sensations in the dark room depend on the retinal region receiving the stimulus. They are at a maximum at the fovea and at a minimum at the retinal periphery.

The number and the duration of visual after-sensations in the dark room are greatly reduced when the light is rendered more diffused (see next paragraph).

The miner's lamp often shines directly into his eyes. He is no doubt disturbed by the incandescent filament and the after-sensations produced by it. The treatment of the outer protecting glass of the miner's standard electric light with hydrofluoric acid, so as to render the glass translucent and to obtain a more diffused light, is greatly appreciated by the miners. They find the work more comfortable and the eye strain less, and are demanding to be supplied with the 'translucent' lamp.

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[1] *First Report of the Miners' Nystagmus Committee. Medical Research Council Report,* Special Series, No. 65.

The intensity of illumination of a white test object in the dark-room, owing to such treatment of the lamp glass, is found to be reduced by 28 per cent. Nevertheless, in by far the majority of persons examined, visual acuity under these conditions was not thereby reduced, but was often actually improved.

(*Manuscript received* 15 *February* 1923.)

THE FUNCTION OF IMAGES IN THE APPRECIATION OF POETRY[1]

BY C. W. VALENTINE.

I. *Introductory: Does psychological analysis injure aesthetic enjoyment?* (pp. 164–166).
II. *Imagery, in the poets, and in appreciation* (pp. 167–168).
III. *Methods and results of first experiments* (pp. 168–171).
IV. *Second series of experiments: the possibility of imagery spoiling enjoyment* (pp. 171–174).
V. *Imagery roused by emotion* (pp. 174–176).
VI. *Images of associated experiences* (pp 177–178).
VII. *The influence of 'unconscious'-memories* (pp. 178–179).
VIII. *Imagery and 'condensation'* (pp. 179–180).
IX. *Substitution and displacement of imagery* (pp. 180–183).
X. *The effects of deliberate imagery*—(i) *during the effort, and* (ii) *subsequently. Images of symbols* (pp. 183–188).
XI. *Imagery and the speed of reading poetry* (pp. 188–189).
XII. *Summary of results and conclusions* (pp. 189–191).

I.

To some lovers of poetry it may seem as irreverent to attempt to analyse the processes involved in the enjoyment of poems as it would be for a man to 'peep and botanise on his mother's grave'; for example Mr A. C. Bradley thinks it would have seemed an impiety to Shelley to break up the unity of a poem for the purpose of study[2]. If the possibilities of poetic experience were as rare as such a visit to the shrine and if the analysis of the process on the whole lessened enjoyment, the psychologist would certainly be open to attack. While, however, it must be admitted that studying the process of appreciation will momentarily tend to lessen the aesthetic enjoyment, it by no means follows that in the long run the analysis will injure aesthetic experience, any more than the literary student's interest in the personality of the poet will injure the aesthetic appreciation of his poetry.

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[1] The greater portion of this paper was read at a Meeting of the Aesthetics Section of the British Psychological Society, 12 May, 1923.

[2] *Oxford Lectures on Poetry*, 159.

It is noteworthy that those who turn to the psychology of aesthetics are themselves keen lovers of beauty itself: and I think that their testimony would be that, taken as a whole, their psychological interest increases rather than diminishes their enjoyment of the art concerned. No doubt the question is one of degree. The continuous absorption of a man in the psychological study of aesthetic processes might bring him to a stage at which he could not enjoy the artistic as art. But if he is in the first place a keen lover of that art, he is not likely to lose that capacity.

The literary critic can hardly raise this objection to the psychological study of aesthetic appreciation without exposing himself to a *tu quoque* argument. For he too would seem to be in danger of going to an extreme. The scholar is apt to find increasingly his interest in *knowledge about* the poet and his work, and in points of textual origins, and, indeed, as Mr Lamborn says, "the knowledge may do something to make up for the inevitable loss of zest that years will bring[1]."

I should offer, however, a similar defence for the literary critic. Granted that occasionally the aesthetic attitude is destroyed by the lapse into the attitude of the critic or scholar, still, on the whole, aesthetic appreciation is enriched. At least it may be urged that by alternating aesthetic and more purely intellectual interest, one prolongs the period of enjoyment of the literature, for the intense aesthetic emotion cannot be indefinitely continued. And even if aesthetic enjoyment were lessened by study, the intellectual interest might more than counterbalance the loss of aesthetic feeling in a comprehensive judgment of values of life as a whole; and a similar argument applies to the psychologist. But for the literary critic one need hardly fall back on this defence. It would, I take it, be generally agreed that criticism, if it sometimes leads away from aesthetic appreciation proper, more frequently, in A. C. Bradley's words, facilitates the poetic apprehension of the meaning just as criticism and analysis of a musical composition "may greatly help the uneducated lover of music to hear more truly the music itself[2]."

It is more doubtful whether any such positive help to aesthetic appreciation is given by psychological study. The attitude of the psychologist is subjective, his attention is turned inwards to the experience as such. That of the literary critic is directed more to the poetical work itself. And aesthetic experience involves something in the nature of absorption in the beautiful object, while attention to the self as experient is antagonistic to it.

Yet even here the question is only one of degree. The critic is constantly verging upon psychological discussion, though he does not usually carry his analysis far. He must constantly appeal by introspection to aesthetic experience as a final test. His writings are sprinkled with references to emotions, feelings, imagery, sensations, and their relations to the poem.

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[1] *Rudiments of Criticism*, 124.

[2] *Op. cit.* 33.

On the other hand the psychologist is also led to study objectively. He finds suggestive material in the nature of the poetry itself. In fact both the psychologist and the critic are really concerned with the *relationship* of the work of art to the appreciator. The ultimate appeal even for the critic is to the existence of aesthetic enjoyment.

All this granted, however, we must admit that the critic moves his attention from this aesthetic experience towards the objective study of the poem, while the psychologist proceeds with the analysis of the experience. The psychologist's aim is understanding of process. That of the critic may be partly to aid the reader to appreciate more fully the poem, and to produce the aesthetic attitude.

These two aims indeed converge in the psychologist who is also an educator. He seeks to aid the appreciation of poetry, but by the understanding of the process of appreciation and not only by the objective study of poetry. The inquiries as to what kinds of poems are enjoyed, what stand the test of time and so on, give us broad principles. But tastes notoriously differ and individual differences appear. It is not merely that some persons seem to lack the capacity of aesthetic appreciation. For the same object may be enjoyed by different individuals for different reasons, and thus we are driven to study individual processes in the enjoyment of poetry.

This method becomes the more essential if it be children we wish to train in appreciation. For literary criticism can confine its attention to the poem only in so far as it can assume uniformity among the minds of individuals. When literary criticism is unconvincing its tendency to ignore individual differences is, I think, the chief cause. But when one comes to deal with children, the wide differences between them and ourselves in experience and development, compel us to pay more and more attention to the personal element in the relationship of mind and poem. And just as the medical researcher will risk losing his life or health, so for the sake of helping to spread appreciation of poetry, the psychologist might be willing to sacrifice, if it were necessary, even some of his own poetic vision in the cause of investigation.

II.

In this paper I shall select for special consideration the function of imagery, especially visual imagery, in the aesthetic appreciation of poetry: and its relation to feeling.

I first became interested in the part played by imagery in poetry, not through my own use of imagery in poetry, which up till then had been, as far as I know, almost *nil*, but through noticing Wordsworth's frequent references to his own intense enjoyment gained from visual imagery. That poets generally have had vivid visual imagery is, I think, usually assumed. Their own use of the term 'imagination' seems frequently to mean imagery, especially visual, with, however, the added modification of creative activity. It is not merely reproductive imagery, for "Imagination bodies forth the form of things *unknown*." Yet this imagination is classed by Shakespeare with the visual hallucination of the lunatic who "sees more devils than vast hell can hold."

Wordsworth, however, seems to draw more explicit attention to the use he makes for his own enjoyment of merely reproductive imagery. I refer to such passages as the following: the well known lines on the daffodils

> They flash upon that inward eye
> Which is the bliss of solitude,

(which though written by Mrs Wordsworth are quite typical of the poet himself).

> Where'er I go,
> Thy genuine image, Yarrow!
> Will dwell with me—to heighten joy,
> And cheer my mind in sorrow.

And perhaps most clearly of all Wordsworth's vivid imagery is shown in his *Lines above Tintern Abbey*,

> These beauteous forms,
> Through a long absence, have not been to me
> As is a landscape to a blind man's eye:
> But oft, in lonely rooms, and 'mid the din
> Of towns and cities, I have owed to them,
> In hours of weariness, sensations sweet.

It is true that in such passages Wordsworth is really speaking of the value of imagery in the recall and enjoyment of natural objects; but it would not, I take it, be disputed that this recall is an essential part of the aesthetic experience of the poet, which he expresses in the poem itself. This in itself is suggestive to us in the attempt to enter into the poet's own experience and to recreate a poem for ourselves.

It would be of great interest to get a detailed account from some poets of their imagery in writing and reading poetry. The intense activity of imagery involved in some creative work, is shown in the following report given me by a young writer whose poems show great promise.

> The sonnet was written, as is pretty obvious, whilst staying in the mountains last August. The vividness of the images was the cause of much restlessness, and I could not sleep till they had been discharged. The first part of the poem was written at 2.30 a.m. When I had written it (in about ten minutes) I could sleep. I finished the poem in the day-time following. The last part of the poem contains scarcely any imagery and took longer to write. I wrote a good deal that August, and generally to relieve the overpowering imagery. I spoke truly when I condemned my vivid imagery as a hindrance to quiet and concentrated thought, a source of continual restlessness. I am not clear as to which comes first, the desire to create, or the image —the latter I think in the two poems here enclosed.

Some literary critics indeed seem to maintain that, even for the appreciator, imagery is the very essence of poetry. For example Mr Greening Lamborn, in his delightful book *The Rudiments of Criticism* says that "To exist as poetry, emotion must be translated into music and visual images[1]." Professor E. Allison Peers, in an account of a valuable experiment on the 'appreciation of poetry by schoolboys,' writes of poems the ideas of which are 'contained in the imagery' as contrasted with others in which imagery is 'not essential[2].'

Mr Prescott in his book *The Poetic Mind* for which the psychologist will, I think, have little but praise, insists rightly that the true test of poetry is 'imagination' (p. 140): but he defines imagination on the preceding page as "the eye of the mind, the mental or ideal counterpart of the bodily eye," and elsewhere (*e.g.* p. 188) his phrasing suggests that he uses imagination as equivalent to imagery.

III.

With a view to discovering the extent to which imagery was of influence in the appreciation of poetry, the experiments here described were begun.

The preliminary experiments were carried out on groups of graduates and graduating students in the University of St Andrews, and second year students in the Training College of Dundee during the sessions 1912–13, and 1913–14. Short poems or selections from poems were read out. The poems were deliberately chosen so as to include a considerable variety, and to justify me in telling each class at the outset of the experiment that some of the poems I thought good and some poor (D and G below). This would encourage independent judgment, which was further ensured by anonymity—the students simply writing 'man' or 'woman' on the paper, or at the most a *nom de plume*. The poems comprised the following selection.

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[1] *Op. cit.* p. 14.

[2] "Imagery in Imaginative Literature," *J. of Exper. Ped.* II, No. 4, 274.

A. Wordsworth, *Lines composed above Tintern Abbey* (first 25 lines).
B. G. Herbert, *The Gifts of God* (Palgrave's *Golden Treasury*).
C. E. Waller, *Lines on a Girdle* (*Ibid.*).
D. A stanza taken from one of some published poems by an acquaintance of mine.
E. Shelley, *To the Moon* (*Golden Treasury*, p. 305).
F. Byron, *There's not a joy the world can give.*
G. A stanza of a poem printed and used for memory experiments.

The poems were read aloud to the class by myself, each poem being read at least twice. The students were then asked to write down—

(i) whether they found the poem very pleasing, pleasing, slightly pleasing, indifferent, slightly displeasing, displeasing, or very displeasing;
(ii) the reasons for their judgment;
(iii) details as to their experience on hearing the poem read.

Care was taken to make no mention of imagery at this stage, lest suggestion should result in imagery which would otherwise not have occurred.

This was followed by a second reading of the poems. In this series the student was asked—

(i) to give again a judgment as to pleasingness, etc.;
(ii) to observe imagery;
(iii) to record any change of experience which might account for the difference of judgment, as compared with the first series. This was partly with a view to ascertaining whether any imagery suggested by the instructions influenced the appreciation of the poem.

These first tests were conducted on two sets of students, numbering in all twenty-one men and twenty-eight women.

Poem A naturally prompted more imagery than the others. Thirteen of the twenty-eight women spontaneously report imagery on the first reading, and four of the others report imagery in the second reading. Of the twenty-one men, seven report imagery in the first reading, and of the others three report it in the second reading.

From the reports it is evident that a few believe they find their chief pleasure in the imagery suggested. Yet that imagery is by no means essential to enjoyment is clear. Two women (or perhaps three—one case of association may include imagery) and six men describe the poem as 'very pleasing' yet report no imagery in the first or even in the second reading when instructed especially to look out for imagery. Still there was evident a decided correlation of imagery and enjoyment. Of five 'indifferents' only one was accompanied by imagery, and one 'indifferent' in the first reading with no imagery became 'pleasing' in the second reading when imagery occurred.

Visual imagery seemed also to be distinctly associated with the enjoyment of poem E—Shelley's *Lines to the Moon*. Each of the four women who report it 'very pleasing' report imagery, and the fifth's reference 'to feelings like those on a starry night in the country' suggest it. Of the men, the two who find the poem 'very pleasing' both attribute their enjoyment partly to imagery, and the other four who report imagery find the poem 'pleasing,' 'pleasing,' 'slightly pleasing' and 'slightly pleasing-pleasing.' The others who report no imagery include five 'indifferents,' two 'slightly displeasings' and six 'slightly pleasings.'

For true aesthetic tests perhaps G at least should be entirely omitted. It produces few judgments other than 'indifferent' and 'slightly displeasing.' If omitted the proportion of the number of images (as shown in the table below) would be considerably increased. I give the figures to illustrate the possible effect of suggestion, in the instruction to look for images in the second reading. The numbers for the twenty-one men are increased in the table by one-third to make them comparable to those of the twenty-eight women.

TABLE I

	Men	Women
No. of first judgments with imagery ...	30	35
No. of first judgments without imagery	165	161
[If poem G omitted]	138	133
No. of cases in which imagery appears for the first time in the second judgment	13	30

It will be seen that in forty-three cases, out of a possible 326, images were reported with the second reading when they had not been in the first. I do not think that these are all cases of suggestion. A few definitely report that imagery did occur in the first reading, but was not thought of till their attention was called to it by the instructions for second reading. In several cases also imagery was said to occur more readily in the second reading when the meaning of the poem was better grasped. Still the figures tend to support my suspicion that suggestion would be of influence, a suspicion confirmed by more recent experiments[1]. The figures also suggest that the effect is shown more by women than by men. But the numbers are too small to generalise upon, and the question is partly one of facility of imagery and not merely of suggestibility.

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[1] This should be borne in mind in considering the results of such experiments as those by Professor Alison Peers, *op. cit.*

An interesting sex difference occurred in reference to Waller's *Lines on a Girdle.* Very few of the women liked this poem. It was reported as 'silly,' 'exaggerated,' 'disgusts me,' etc. Only one out of twenty-eight found it 'very pleasing,' and little imagery was reported—only with four judgments, two of which were second judgments, 'pleasing' and 'slightly pleasing' taking the place of 'indifferent' without imagery.

Some of the men also take a similar critical attitude to this poem, but six of the twenty-one find it 'very pleasing,' six others 'pleasing,' with nine reports of imagery.

IV.

After the two experiments described I made a third series in which I read to two other groups of students, at St Andrews and Dundee, poems in which one might expect imagery to tell more heavily. The verses selected were:

A. The first stanza of Shelley's *Ode to the West Wind.*
B. The fourth „ „ „ „ „
C. Wordsworth's *She dwelt among the Untrodden Ways.*

Each poem was read aloud by me three times, after which the students were asked to give their judgments on the poems, and their reasons, but in my instructions no mention was made of imagery. While the students were writing their judgments I again read to them the lines a fourth and later a fifth time. When all the writing was finished the students were asked explicitly to give an account of the imagery experienced, and to estimate the extent to which imagery had helped or hindered the enjoyment of each of the poems. If it helped 'very greatly' three was scored, if 'greatly' two, if 'slightly' one, if it hindered 'slightly' −1 and so on. Of the first group of students, nine in number, these are the scores.

TABLE II

Indicating the extent to which imagery helped appreciation with three selections of poetry. Score: 3 = very greatly; 2 = greatly; 1 = slightly; −1 = slightly hindered.

A	B	C
2	1	3
3	2	3
2	−1	1
2	1	3
1	3	1
1	3	1
3	2	2
2	−1	3
3	1	2

It is notable that, out of twenty-seven units, the appreciation of nine was said to be helped 'very greatly' and eight 'greatly' by imagery —and that too when imagery was only mentioned after the poem had been read and judged. The table also twice suggests, however, that while imagery may help, it may also hinder appreciation—even for the same person.

This was still more clearly shown in similar experiments with the second larger group of students. In this I asked the students after finishing their spontaneous report, but only after the last reading, to recall as far as possible the images they had had in reading the poem, to state what was imaged and to give the marks three, two, etc. to *individual images*, according to the way in which they had been found 'very pleasing,' 'pleasing,' etc. The results were as follows: the pseudonym chosen is given.

TABLE III

Scores of individual images experienced in hearing read three selections of poetry. Score: 3 = very pleasing; 2 = pleasing; 1 = slightly pleasing; −1 = slightly displeasing, etc.

	A	B	C
	First stanza of Shelley's *Ode to the West Wind*	Second stanza of the same	Wordsworth's *She dwelt among the untrodden ways*
Turvy	3 1 3 3 3 2 3 3	1 3 3 2	3 3
Patience	3 3 3 2 2 3 3 3	2 3 3 3 3 3 2	3 3 3 2
Pansy	None reported	3 1 2	None reported
Bluebell	3 1 1 1	1 1	3 3 2
Dixie	1 0 0 (grave and corpse very vivid images)	3 3 2 2	None reported
Kelly	3 3 3 2 0 −1 (grave) −2 (coffin)	Not completed	Not completed
A.	−1 −3 −1 −1 −1 −2 (but less displeasing in last reading)	0 −3 −1	—
Dante	None reported	3 1 1 3	None reported
Serious	3 2 3 −1 (vivid image of corpse)	2 1	None reported
One	3 3 2 2	2 2	—
Firth	3 3 3 3 3	2 3	−1
Tune	2 3 3	2 2	−1
Yum Yum	3 3 2 1 3 3 3 2 3	1 1 2 3	—
Cinderella	3 2 1	−1	2 3
R.	3 3 2 −1 (pestilence) −1 2	—	—
Snowden	3 3 3 1 0 (corpse)	3 3 3	3
Peggy	2 2 1 1 1	3 3 3	3 2 3 2
Mouse	3 1	3	1 1 1 1
G. Watt	3 −2 3 −3 −2 (death, desolation, bare, branching; images spoilt enjoyment)	3 2 3 −2 3 (*man mourning*)	—
Topsy	3 −1 2 3 1 2 1	1 2 2 1	—
Expt.	−1 −3 −1 −1 −1 (leaves spoilt, trees bare. If I had had no images I might have enjoyed the poem much more)	0 −3 −1 (clouds suggesting rain, waves, shipwreck, leaves curled up)	−2 (cypresses moving)

To one who, like myself, has little visual imagery and that only vague, it is enlightening to find so large a proportion of a group having so great a number of vivid and highly pleasing images, and these, too, clearly recalled.

Yet there is evident also the possibility of imagery spoiling enjoyment—by making certain details, unpleasant in themselves, stand out too clearly. As elements of the general significance of the poem, these details are right: they add to the general tone. As soon, however, as such elements as the 'grave,' 'corpses,' etc., are dwelt upon for a moment in some degree of isolation, breaking away from the unity of the poem, their characteristic feeling-tone becomes too predominant. And one of the occasional results of imagery seems to be this undue emphasis of the detail, out of proportion to its ideal place in the poem. Thus in my own reading of Wordsworth's *Ode to Immortality*, "Let the young lambs bound," very vaguely imaged as a part of a bright spring landscape, gave a general feeling of gaiety. On another occasion the skipping lamb was more definitely visualised as an isolated picture, and the effect was ludicrous and spoilt the line for me. A very definite image however need not hinder, it may greatly help when the objects imaged are main factors in the poem, *e.g.* the scenery in a nature poem. Some subjects in the experiments reported a clear, definite and pleasing image regularly recurring with the same poem, *e.g.* image of a tree in reading "Oh! to be in England!" But definiteness here does not break the essential unity of the poem. I think the danger is greatest where symbolism is prominent. Thus take Shelley's lines,

> Here lieth one whose name was writ in water.
> But, ere the breath that could erase it blew,
> Death in remorse for that fell slaughter,
> Death, the immortalizing winter, flew
> Athwart the stream,—and lo, time's printless torrent grew
> A scroll of crystal, blazoning the name
> Of Adonais!

In early readings I had a very faint image of a vast wild river, of a sudden flash across it, of something vaguely typical of the spirit of death moving on the wings of a freezing blast—all this added to the thrill produced by Shelley's lines. But a very slight emphasis of the image, a more definite image of a wide river, or of the name 'Adonais' in big letters in the ice, and the magic of the poem is lost. Here emphatically, when all goes well the image seems to be the mere *effect* dependent on the feeling, it does not add much to the feeling.

There is an element of the sublime here in Shelley's conception of death. And as A. C. Bradley points out (quoting Burke) the sublime is sometimes dependent on obscurity and vagueness. An unknown object sometimes stimulates fear the more, and in such poems, Bradley asserts, pictorial illustration would spoil the effect.

Similarly we may agree that visual imagery, performing some of the functions of pictorial illustration, may also lessen the effect of the sublime dependent on obscurity and vagueness, if that object is too clearly imagined.

V.

In the earlier St Andrews experiments, in the case of Byron's *There's not a joy the world can give*, even when there is high appreciation of the poem, the imagery usually seems only incidental. Several times it is reported after the second reading, as having been experienced in the first reading but not noted, as enjoyment was not associated with it. In fact with this poem, we get hints of a general principle—that imagery may tend to accompany strong feeling even when that imagery does not add appreciably to the feeling: or that imagery may be primarily a consequence of feeling, even when it is also a cause for further feeling.

This tendency would seem to be most evident in the mind of the poet himself, whose feelings stir imagery, which again works on the feelings. In a similar way does fear prompt to illusion or hallucination, and then again rouse further fear. It is a disputable point whether a little child is afraid in the dark when and because she 'sees things,' or whether she sees things because she is afraid: personally I think both are true. It is this causation of illusion or imagery by feeling which I think Shakespeare has in mind in the lines,

> Such tricks hath strong imagination,
> That, if it would but apprehend some joy[1],
> It comprehends some bringer of that joy;
> Or in the night, imagining some fear,
> How easy is a bush supposed a bear!

The tendency for facile and vivid imagery to accompany the poetic mind seems clear. Even in the perception of sensible beauty the poet's mind, as the artist's, is to some extent creative. So Wordsworth writes—

> Therefore am I still
> A lover of the meadows and the woods,
> And mountains; and of all that we behold
> From this green earth; of all the mighty world
> Of eye and ear, *both what they half create,*
> *And what perceive.*

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[1] If I am right then Verity utterly misses the point in his note on 'Some joy,' which runs 'some object of joy.'

If this happens in perception, *a fortiori*, it is still more likely to happen in imagination.

A present-day writer whose poems have received high praise, said, in discussing this question with me, that it seemed to him self-evident that in a state of emotion imagery was greatly stimulated. Since then, I have carefully noted my own experience, and in the case of at least two different emotions, I am convinced that this is true. In one case even the deliberate raising of an emotion by dwelling on certain thoughts, produced vivid and active images which I had quite failed to get before by deliberate effort. Again, one cannot read Shelley without believing that the poet was sometimes so dominated by an emotion that vivid and rapid series of images were roused by the feeling, and that the feeling is the primary thing, and the images secondary and expressionary. By the calling up of such images in the mind of the reader the poet may produce a similar state of feeling, in so far as such imagery may be bound up with 'the general feelings of mankind.' Yet in the mind of the reader himself, in so far as a genuine poetic experience is achieved, in so far as a dominant emotion is roused, images congruent with the mood roused by the poem, but not necessarily representative of its content, may arise.

Thus the question of imagery in poetry is complicated by the problem of the *creative imagery activity* of the reader under the spell of the poem; it is not merely a question of the influence of images of things directly described in the poem.

It is a mind which combines a strong aesthetic sensibility to the beauty of poetry with facile imagery, in which one finds this occurring most clearly. I may illustrate by the case of M. M., an advanced student of mine at Birmingham University, who was one of the best teachers of English I have ever known. To one watching her teach it was evident that even in the schoolroom she thrilled with the beauty of the poem she was dealing with. She has also vivid visual imagery. She has had a long training in psychology, including experimental work, and her introspection can be relied upon. I asked my M.A. Degree class of 1922, of which she was a member, to choose one of their favourite poems and to write as full an account as possible of their experience in enjoying it. I give some quotations in reference to imagery from the record of her "Psychological Analysis of my Appreciation of F. Thompson's poem *The Daisy*," with first her final note.

When I first read the poem I liked it because of the vivid visual imagery aroused, the beauty of the scene and of the child, the change of mood in the poet, and the exhilarating feeling the first few verses gave me, as though I were on a high place on a warm sunshiny day with a sweet clean sea-breeze blowing into my face. Later I read something of the poet's life, and saw his photograph, and this has increased though somewhat altered my appreciation; the poet figures more largely in the visual imagery. The photograph reminded me of the face of my own father. Thus in the visual imagery the actions and appearance of the poet are always characteristic of my father[1].

Where 'mid the gorse the raspberry Red for the gatherer springs, Two children did we stray and talk Wise, idle, childish things.	*Imagery.* Uneven ground, vivid picture of yellow gorse flowers and red raspberries. The poet tall and black against the sky holds the hand of the little child as they walk. Both figures are bent forward a little as they walk against the wind.
She listened with big-lipp'd surprise, Breast-deep 'mid flowers and spine: Her skin was like a grape, whose veins Run snow instead of wine.	*Imagery.* Poet lies full length on his back on the grass, with trilby hat beside him and looks up at the laughing child—I have seen my father lie so many a time in the summer.
The fairest things have fleetest end: Their scent survives their close, But the rose's scent is bitterness To him that loved the rose!	*Imagery.* A very beautiful, sad woman stands in a room smelling a crushed red rose. The room is dark; a fire is burning in the grate. She is standing against the mantelpiece as though for support.
She looked a little wistfully, Then went her sunshine way: The sea's eye had a mist on it, And the leaves fell from the day.	*Imagery.* Child turns from the poet and skips away in the direction of the sea. A cloud covers the sun and the picture is left grey and cold. For last line—a huge tree, big as the world, great leaves falling—like sycamore leaves Tree has a twisted trunk.

Most of this imagery is evidently suggested by descriptions in the poem. But careful examination will show how in a number of cases the feeling of the poem has carried the reader beyond the material actually described, *e.g.* the 'huge tree, big as the world, with a twisted trunk,' the 'poet walking away with bent head,' 'the beautiful sad woman smelling the crushed rose, standing in the dark room, and leaning against the mantelpiece for support,' all fitting in with the feeling and mood of the poem, but not items described in the poem[2].

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[1] In a further explanatory letter M. M. wrote: "I confuse or identify in my mind a photograph of Francis Thompson with an old portrait of my father. They are attired very similarly, viz. in old-fashioned black suits which reminded me of clerical garb."

[2] Professor Allison Peers noted similar 'creative' imagery among boys; *op. cit.*

VI.

It is important to distinguish between such imagery immediately called up by the poem and enjoyed as part of the poetic experience, and imagery which is called up by less immediate association with the images proper or the ideas of the poem. In the poem above the suggestion of the father, the close blending of his image with that of the poet is a good example of an intermediate type. More remote associations are exemplified in the following record of a student in a test which I may distinguish as the Favourite Poems Test (Birmingham, Graduate Class 1921-22). Students were asked to select two of their favourite poems and to write an analysis of their experience after reading the poem. E. H. Y. chose as one of his favourite poems Wordsworth's *Lines Written in Early Spring*—

> I heard a thousand blended notes
> As in a grove I sat reclined.

He refers to his life as a boy in the country, and his early love of nature. He writes,

> I seem to live through, by virtue of my own experience, the instances that the poet gives of the common beauties of nature. I get mental images, visual, auditory, and to a lesser extent olfactory and tactual, of the natural objects mentioned by the poet, and I see, as it were, through his mind's eye. If I read slowly, however, and take each image separately, I get a mental image from my own experience, distinct from the context, but suggested by the poet's words. There is a pleasure associated with those personal images, which, I think, adds to the pleasure associated with the images in the poem itself.

With facile visualisers it is surprising how readily such supplementary images crowd in, even when the poem is read at ordinary speed, and with no searching for associated images. We cannot assume that the pleasure associated with these images is properly in all cases part of the aesthetic enjoyment of the poem, any more than associated ideas are. Yet sometimes such images seem so intimately associated with the images proper to the poem, or they so seem to illustrate as it were the general idea of the poem, that they become a true part of the aesthetic experience. They are then comparable to the intimate, unavoidable, permanent associations of a musical chord with its suitable successor, or to the general or unconscious associations with colours, 'fused' associations as Mr Bullough has called them, rather than to the more remote associations which are often referred to by unaesthetic persons in giving their reasons for liking a picture or a piece of music, *e.g.* when a landscape is liked 'because it reminds me of a happy holiday by the sea' or a musical composition 'because it suggests soldiers marching.'

In some experiments on the appreciation of musical intervals, I found that associations best named by the term 'fused associations' were accompanied, twice as frequently as 'non-fused' associations, by judgments 'very pleasing,' a good indication of aesthetic enjoyment[1].

Similar types (fused and non-fused) evidently occur among the associated images in the appreciation of poetry. And the non-fused associations do not tend as a rule to be accompanied by such pure aesthetic appreciation and enjoyment as in the case of the more fused associations.

VII.

Even when such images (fused or non-fused) do not occur (because for example there is no pause or reflection on the ideas and images proper to the poem), it seems to me likely that the experiences they represent may have an important influence on the enjoyment of the poem.

This brings us to the question of 'unconscious influences.' It is sometimes a refuge of failure to fall back on the unconscious as an explanation. Yet when conscious analysis fails to explain at once why a poem has an intense emotional effect upon one, and when on reflexion and by the getting of associations connected with elements of the poem, one can see that past experiences might account for the emotional effect, it is not unreasonable to suppose that they have at least some influence, when they are not suspected. The question is on a par with the interpretations of dreams by getting associations with their elements, a process which does, in at least a substantial minority of cases, lead to a possible explanation of an otherwise incomprehensible dream.

To illustrate by reference again to colours. We find that associations play an important part, especially with some people, in determining the attitude adopted towards a given colour. There are probably deep-set associations whose origins we have long forgotten. "The pleasure of a given blue may be due to the faint revival of many happy experiences under blue skies, though we may not remember and may not even think of a blue sky. Experiments have shown that we may have a feeling, similar to one we experienced when we saw a given object, even when that object itself is not recalled. Thus we cannot necessarily conclude that vague associations are not at work when a colour pleases us, merely because we cannot trace them[2]." The rousing of a vague feeling by some idea or object, which does not seem to explain it, is often seen to be due to some past experience, sometimes going back to childhood, which roused a similar feeling and which in some way was linked with the object or idea. The intense feeling tone of some dreams can be explained in this way. Wordsworth himself constantly refers to the accumulated influences of and feelings of boyhood, feelings which may be roused without very definite memories, and which add to aesthetic enjoyment. He speaks of 'feelings of unremembered pleasures,' of 'obscure feelings representative of things forgot,' of

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[1] See C. S. Myers (with contributions by C. W. Valentine), "A Study of Individual Differences in the Attitude towards Tones," this *Journal*, 1914, VII.

[2] C. W. Valentine, *Experimental Psychology of Beauty*, 2nd Ed. 20.

> Those first affections,
> Those shadowy recollections,
> Which be they what they may,
> Are yet the fountain light of all our day.

In my own enjoyment of certain poems I find that, as appeared in the case of several of my most reliable subjects, an unusually strong feeling is more explicable on the assumption that the images called up are connected with long past experiences in which feeling was intense. For example, Kingsley's poem *The Sands of Dee*, has a much greater effect upon me than I should imagine it should have, judging the poem calmly and critically. Now though I usually visualise very little in reading poetry, in the case of this poem I always have a vivid image of the wide Dee estuary seen first in adolescence. But why the haunting, creepy, fearsome yet fascinating loveliness attached to the image? No doubt it is due in part to the immediate impression of that scene. But this impression can be traced further back. After an early childhood spent in great cities, almost my only early emotional recollections of nature are those of the sea-shore, not of the gay summer resort, but of the wide bare stretches of lonely sand and cliff. And in reading the poem the image of the Dee shore changes without a perceptible break with images of the types of scenery of the Durham coast. The types of scenery, and the corresponding images, are quite different, but the feeling is entirely congruent[1].

VIII.

The variation of imagery with similar feeling is a like phenomenon, I would suggest, to 'condensation' which is in itself a further illustration of the influence of unconscious or only vaguely conscious elements. Freud has pointed out that in dreams one person or object is often really a blend of two or several towards whom the feelings are similar. Thus the emotion aroused by the two persons may be 'condensed' round a symbol which stands for both of them. One of the many examples in poetry of condensation is when Swinburne addresses the sea,

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[1] Since writing the above I have revisited for the first time for thirty-six years the Durham coast referred to. To my surprise I found that there were far wider stretches of sands than my images suggested. The similar feeling-tone remained though the image degenerated.

> I will go back to the great sweet mother,
> Mother and lover of men, the sea.

Throughout the poem one feels alternating the suggestions of the protective mother love ("Save me and hide me neath all thy waves") and of the more passionate love of a mistress.

All metaphor is a similar blending and association. The feeling element is not always strong but probably the most truly poetic bond is the bond of feeling rather than of similarity. There would seem little external resemblance between the sea and its shore on the one hand, and the lover and his mistress on the other. Yet in Shelley's lines,

> Where the blue heavens bend
> With lightest winds, to touch their paramour;
> Or linger, where the pebble-paven shore,
> Under the quick, faint kisses of the sea
> Trembles and sparkles as with ecstasy,

we realise that the languorous feelings stirred by the sight of the sparkling sea lapping the sunlit shore fit in well with the suggested feelings of the lover.

IX.

In further experiments with a graduate class, one subject speaks of the effect of reading Francis Thomson's *Mirage* and of the vivid and sinister images of the shadows, and of 'rusty winged Death,' the pleasure in this poem in which there is scarcely any intellectual interpretation of the words, being 'almost entirely sensuous' of the words and images, a combination which almost exactly describes my appreciation of Coleridge's *Kubla Khan*, the only poem my enjoyment of which is to any great degree dependent on imagery.

The regularity of imagery is emphasized by another who writes, "I always have an exceptionally definite picture of the scene described; I see always exactly the same tree and imagine what the blossoms and dewdrops would feel like if I stood under that tree."

In the Favourite Poem Test (Birmingham Graduates Class, 1921–22) one student after showing that imagery in a poem of Shelley's could 'carry' feeling even when the image is vague, writes, "The subject matter is of minor importance, for it is entirely banned from entering the mind by the melody of the words,"—a hint of *competition* between thought and sensuous enjoyment in poetry. That this student is not incapable of appreciating thought in poetry is shown by her report in her second selection (Wordsworth's *Ode on Intimations of Immortality*) in which she says, "the outstanding feature is the depth of thought."

This leads me to consider the 'substitution' or 'displacement' of imagery in poetic appreciation. In a number of the group experiments on poetry and imagery and on the basis of my own introspection, I found ground for suspecting that when very vivid imagery occurred, especially after suggestion, it took the place of something else. I had myself found, when joining in the Dundee experiments, that a mild effort to visualise led to imagery which might be enjoyed, but which seemed to displace something else. In some later experiments with a Birmingham graduate class I find these reports: "The auditory images are obscured by the strength of the visual." Another reports that visual and auditory images become prominent in successive stages. The appreciation of the strong and musical rhythm seems "an alternative to visual imagery" to another who writes, "I find that if rhythm enters in with musical words I pay attention to them, and do not visualise at all, but when there is no musical rhythm, visual imagery of scenes familiar to me associated with the words is very pronounced." Another writes, "I get more imagery and this is pleasing, but did it help or was it a substitute for something?" This last student was an Honours graduate in English with a previous training in psychology, and one of my very best students in experimental psychology. After reading the selections used in an earlier experiment in the appreciation of poetry[1], which were the first lines of Wordsworth's *Lines written above Tintern Abbey*, and the five first stanzas of Grays' *Elegy in a Churchyard*, he wrote as follows:

(*a*) Practically speaking, I have no imagery at all while reading these extracts, though striking phrases, 'blazing hearth,' 'shrill clarion,' 'incense breathing morn' make images so to speak to stir in the depths. Yet I find on completing the reading that I have appreciated them as if by unconscious reference to experience. I can, if I so desire, produce most of the images as I read, however. I fancy that I should resort to imagery if the description were complex.

(*b*) On a second reading I find I have a continuous train of images. Which is normal I cannot at the moment say. I shall try again with other extracts after an interval of a day or two.

(*c*) On further experiment I find that sometimes I have a wealth of imagery as I read, while at other times in reading the same poem, I have very little, though I enjoy the poem just as much. I think the latter is more usual.

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[1] *I.e.* Experiment IV in the writer's ***Experimental Psychology and Education.*** Incidentally it is an interesting point that this student, a specialist in English, and one with a genuine capacity for psychological analysis, should not have been already aware at this stage of his usual habit as to imagery in reading poetry.

Elsewhere we have had ample evidence that a given poem may be enjoyed by some with little or no imagery, though others attribute the chief cause of their enjoyment to the imagery suggested. In the former case it may be that other factors, such as rhythm and meaning have relatively greater influence: or it may be that the words themselves, even in a piece descriptive of nature, carry the feeling-tone associated with natural objects of beauty as vividly as do images in the case of others, just as imageless thought in some is as effective as is thought rich in imagery in others. In the latter case, meaning sometimes only seems to be complete when expressed partially in imagery. And even in poetry this may happen also. Indeed one subject wrote "I can only get a clear grasp of the sense when time is given to visualise[1]."

In subjects in whom imagery is by no means so essential we find that something in the way of substitution or displacement may take place when imagery enters. And this is indeed not surprising on general grounds. The scope of attention at any given moment is limited. Not many elements can be prominent at one time, or if they are they must be blended into or apprehended as a close unity. From this point of view images which though congruent in feeling tone are not also truly representative of the subject matter, are inferior. When, however, images are truly representative and also free-flowing, some persons seem able to apprehend them without losing any of their sensitivity to sound, rhythm and meaning.

One would expect that the more facile imagery is the more readily, on the whole, will it be used in poetry. It may be in consciousness even if it does not receive much attention, just as visual impressions of the walls of my study are present to my consciousness as I write, without at this moment being attended to: and yet the fact that they are green and the wood-work white has, as I have found from experience, a definite effect on my spirits as compared with dark walls and maroon or yellow-brown paint, which always seem the favourite colours of one's predecessor in a house. It is in such a way that, in my own case, vague imagery seems to be of influence in the reading of poetry. It is only in the 'margin' of the field of consciousness. Similarly S. W. M. reports "rhythm is in the focus, meaning and combination of words in the near margin, and general imagery in the subconscious field": and F. M. R. "There was a vague 'feel' of the image—and the feeling that the image was on the 'threshold' and could be produced at will."

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[1] Cf. the fact that, as I have observed in my own case and in several others, the meaning of words in reading vanishes when the accompanying motor and auditory images are repressed.

Some reports make it perfectly clear, however, that the mere capacity for vivid visual images is no guarantee of their being used even in the reading of nature poems. The case of P. above is one. A still clearer case is that of D. L. an artist colleague who took part in experiments in my M.A. and M.Sc. class (1922–23). She has a capacity for vivid visual imagery, freely used in connexion with her painting: and she combines with her artistic ability a marked capacity for psychological analysis and introspection. Concerning her appreciation of poetry she reports that she derives practically no enjoyment from visual imagery.

> Poetry (she adds) appeals to me principally in its musical and rhythmical aspects. I rather wish to read it aloud, or if read silently, it is accompanied by strong aural imagery. I am at a loss to understand those who attribute a large part of their appreciation of poetry to visual imagery of any kind, whether illustration of the subject or due to personal associations. If I had to illustrate a poem I should probably have to invoke consciously a visual image and should prefer the freedom to reinterpret the theme and form and colour rather than be bound to illustrate the words in a literal sense.

Her own paintings by the way are usually symbolical.

X.

I turn now more particularly to the question of the deliberate attempt to encourage imagery in the reading of poetry.

It will be remembered that in the St Andrews and Dundee experiments spontaneous records were asked for before imagery was suggested. As the result of frequent testimony from students and others and from my own experience, I became convinced that the attempt even merely to observe imagery, without any effort to rouse it, does in some cases tend to rouse imagery and to direct attention to it somewhat, and in some cases to spoil the enjoyment of the poem. It was still more evident that such spoiling might occur in actual attempts to image, and, on the basis of supplementary tests in which individual students tried deliberately to get images while reading the poem, I drew the general conclusion that "In the case of those whose imagery is weak a deliberate effort to visualise causes delay in the reading of the poems, and the rhythm necessarily suffers[1]."

As we admitted in the introduction, it is *a priori* likely that any attempt to do anything during aesthetic appreciation, other than attend to the object, will lessen the enjoyment. Perhaps visualising elements of the poems may be regarded as really part of the absorption in the object. But if it involves any appreciable effort it is inconsistent with the easy attention and unity of attention characteristic of the aesthetic attitude.

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[1] *Experimental Psychology and Education*, 1st Ed. London, 1914.

This tentative conclusion as to the harmful effect of a deliberate effort to visualise when reading poetry finds definite support in the results of some interesting experiments by Dr Olive Wheeler[1]. Forty-two of fifty students found that the mere noting of imagery as it occurred in a second reading of a poem resulted in an increased appreciation of the poem in the third reading. But when a deliberate attempt was made to image, thirty-seven of the fifty students found it harmful. It is not clear whether this was found to be the case in the reading which followed the one in which there was an effort to visualise, or only in the reading during which the effort took place. Apparently it was the latter. If so this is quite in accordance with the principle mentioned, namely that ease of mental activity and absorption of attention are essential for aesthetic appreciation.

This explanation by reference to the principle of 'facilitation of attention' is quite consonant with the very suggestive one given by Miss Wheeler as to the spoiling of the 'duration' of the poem. Indeed the two are probably intimately connected. There must be duration, or, as I should prefer to call it, continuity of poetic experience, in order to avoid a breaking away of attention to something other than the poetic thoughts and images themselves. If this happens the spell is apt to be broken, and this may be caused by the effort to image, just as the spell of the *Ode to the West Wind* was broken for me by a sudden reflexion that there were resemblances to some phrases of Wordsworth.

Dr Wheeler points out some educational consequences of such interruptions, rightly warning against the dangers of the method of exposition, "which is too critical or analytic, and therefore disturbs the duration factors[2]."

She seems to conclude from her experiments that the deliberate image-forming method in which children are encouraged to get different images of things mentioned in the poem is bad, though I gather she would approve of the slighter calling attention to it involved in the mere suggestion that any imagery should be observed. We have seen reason to believe that even the injunction to observe imagery may result in increased imagery and, though usually this is accompanied by increased enjoyment, it sometimes seems to spoil enjoyment.

On the other hand, while it seems clear that deliberate attempts to image will usually interfere with the enjoyment of the poem at that reading, it does not seem certain that it may not *in the long run* increase the enjoyment; especially of those who have neglected to use an existing capacity for imagery, as is sometimes the case. I am quite confident, for example, that my own sharing in my students' experiments, and the attempt to study imagery in reading *Lines above Tintern Abbey*, led to the revival of images in *subsequent* readings, and added to the enjoyment of the poem.

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[1] "An Analysis of Literary Appreciation." This *Journal*, XIII, No. I.

[2] *Ibid.* 229.

With a view to testing this, I carried out some further experiments with members of an advanced class in experimental psychology. These latter in most cases were in the third year of a course for the M.A. and M.Sc. degree through each year of which they had had a course in psychology, including experimental work. The members of the class were mostly experienced teachers, including three members of the University staff (Education Department), and five Training College lecturers. The poems selected for the experiment were:

A. Wordsworth's *Sonnet on Westminster Bridge*,

B. Wordsworth's *Lines composed above Tintern Abbey*, ll. 24–51.

This experiment had three stages.

1. The poem A was read aloud by myself, each hearer being asked afterwards to record his judgment on the poem, ('very pleasing,' 'pleasing,' etc.) and the extent to which imagery was of influence.

2. The poem was read a second time much more slowly, students being asked to attempt to get appropriate images.

3. The poem was read a third time at a similar rate to the first reading. Students were asked again to record judgments ('very pleasing,' 'pleasing') and to say to what extent, if any, increased imagery as a result of practice was of influence; but no effort to image was to be made.

A similar procedure was then followed as to Poem B. The time taken for the first reading was 1 min. 18 secs., and for the third 1 min. 15 secs.: for the second about two or three times as long.

The class numbered nineteen, about half being men.

The results were as follow:

Poem A.

Number who found practice of imagery helpful to enjoyment of the third reading...	7
Number who found that it partly helped and partly hindered ...	2
Number who were not affected one way or another	10

Poem B.

Number who found practice of imagery helpful in (3)	2
Number who found practice of imagery partly helpful and partly a hindrance in (3)	1
Number who found practice of imagery only a hindrance in (3)	2
Number who found practice of imagery had no effect	11

A considerable difference will be noted according to the poem. Imagery was of little value in the essentially reflective philosophical lines in B.

Before giving detailed reports we may take the results of a similar experiment with a graduate Diploma Class, fifty in number:

Poem chosen: Wordsworth's *Ode on Intimations of Immortality*, first three stanzas. (In this experiment separate reports were made after each stanza was read.)

Number who found practice of imagery only helpful in third reading	18
Number who found practice of imagery only a hindrance ...	0
Number who found practice of imagery partly helpful and partly harmful	15
Number who found practice of imagery had no effect	17

I give first an example of cases in which the image practice was decidedly of value.

W. H. L. an honours graduate in English, whose predominant imagery is visual, which is very vivid, noted especially the increase in auditory imagery in the third stanza, the visual imagery remaining vivid. In this reading the enjoyment of the poem was distinctly increased by auditory as well as visual images. D. G. and A. H. P. report similarly.

> M. D. Y. The effort to image led to a greater enjoyment in the second stanza. Although I knew the poem very well before I imaged things in a very much clearer way than I had ever done before.

H. S. whose first spontaneous imagery is auditory, visual imagery not contributing much to enjoyment, reports as the result of the practice period:

> Visual imagery much stronger and more helpful in appreciation, auditory imagery seemed obscured by strength of visual imagery [another example of substitution]. In the third stanza both are strong and all seemed to add wholly to enjoyment.

Students were not asked to report on the second reading (deliberate imagery practice) but a few did. Their report usually bears out what Dr Olive Wheeler found, and what I had surmised from the Dundee experiments, that, as one wrote: "the effort to visualise made the poem more disjointed, and I got irrelevant imagery." But one of my most reliable subjects enjoyed the poem more in the second reading through having time to visualise fully.

The injurious effect of deliberate imagery seemed in some cases to continue in the third reading. Thus "Imagery seems to assume undue proportion" (E. E. U.). "Irrelevant images got in second reading persist. They remain 'disjointed' instead of, as in the first reading, 'melting into each other'" (W. N. G.). "More distant associations called up in second reading persist," *e.g.* image of "lonely room" of student in which she had felt homesick, "took the mind away from the poem."

My colleague F. M. R. writes (re *Sonnet on Westminster Bridge*) Stage III:

> Less pleasing as a result of deliberate image practice, though there was a 'carry over' of some relevant images which were pleasant in Stage II. There had been several ludicrous images produced: these did not occur, but the feeling that they were there did occur and caused the poem to be less pleasing. I think the 'carried over' images might have caused the poem as a whole to be more pleasing if they had become 'background' and if the others had not occurred. I feel I shall never read the poem without being likely to see instead of sleeping houses, a hen with head tucked under wing, and chicks around her, and a down-at-heel man with head almost between knees slouching past the bridge. Relevant images transferred were: sunlight on valley, rock and hill and the calm water, which, though imaged in II as the Malvern Hills, seemed to take their place quite naturally afterwards in the poem, though not as Malvern Hills, rather as a more general view.

D. M. L. writes:

> I was conscious only of a vague colour-tone as of an evening sky, with perhaps a suggestion of etherialised smoky chimneys, but none of this was sufficiently distinct to attract my attention if it had not been asked for.
>
> Third reading very pleasing. There was an increase in imagery but I do not think it added to the enjoyment as I was bothered by the fact that one cannot see all the things described from Westminster Bridge—certainly not fields! The colour was changed by realisation that the scene was in the morning, not evening light. I think this curious mistake about the lighting shows how little value the exact imagery has for me. I feel increasingly certain that sound and rhythm, rather than imagery, means poetry for me.

M. E. M. (*Sonnet on Westminster Bridge*) who as stated before gained on the whole great enjoyment from images in the practice period, imaged 'a huge heart—horrible.' She also writes:

> The sense of strong sunlight more vivid, though the attempt to visualise the sea on rock and hill seemed to bring up an irrelevant picture which detracted from unity. Also the comparison is detrimental to my picture of the town. The poet enters more into the imagery, a tall black figure contemplating the scene. This enhances my enjoyment.

I had a similar experience in trying to visualise on reading the same sonnet.

> I had not before had any image other than a vague one of Westminster Bridge. This time I not only imaged the river and silent buildings, which enabled me to enter more vividly into the poet's feelings, but I had images corresponding to the lines,
>
> Never did sun more beautifully steep,
> In his first splendour valley, rock, or hill.
>
> This individual image, based on a view of Welsh mountains, was in itself refreshing. But it came perilously near to making me less receptive to Wordsworth's suggestion

that the beauty of the early morning light on London surpassed it, so that it was difficult to say whether the poem as a whole gained or lost.

We may draw the following conclusions in reference to the effect of deliberate attempts to image.

1. While the actual process of deliberate imagery is harmful to the enjoyment of poetry, the after effects of such a practice tend, with many individuals of the type tested, to increase their enjoyment of the poetry, if it be such that the content of the poem naturally suggests the vision of things pleasing in themselves.

2. There is a constant danger, however, for objects visualised deliberately, and not as the spontaneous result of the feeling roused by the poem, to break the unity of the poetic appreciation, by their incongruity or by the over emphasis of details.

3. While facile visualisers have the aesthetic attention less interrupted during the deliberate attempt to visualise, they are not exempt from the danger mentioned under (2).

4. The value of the practice period is probably greatest for those who have fairly good visualising capacity but have not been made aware of it or at least have not used it appreciably in poetry.

XI.

As a check upon the influence of speed upon imagery (apart from any effort to image), I carried out two further simple experiments with the Graduate Class. Selecting a short poem (*Golden Treasury Series I*, No. LXXVII) I read it over to the class first slowly (40 secs.) asking them to report on (i) enjoyment and (ii) imagery. Then followed a fast reading (20 secs.) followed by similar enquiries. Later a second poem (*Golden Treasury Series I*, No. LXXVI) was twice read. This time the fast reading (30 secs.) came first, followed by the slow reading (50 secs.).

Neither poem is a nature poem or of such a type as one would suppose would be likely to rouse much imagery, certainly not so many images as to crowd upon one another. The results were as follows: the class numbered about fifty.

Poem A.

Number who found imagery helpful in slow reading	36
Number who found imagery helpful in fast reading	15

Poem B.

Number who found imagery helpful in slow reading	24
Number who found imagery helpful in fast reading	2

The particulars indicate that often the meaning of the poem was not fully grasped in the first reading which tended to lessen imagery. Yet in spite of this far fewer subjects found imagery helpful in the second fast reading of poem A than in the first slow reading. With most it seems that the first reading does not give the imagery time to develop. I quote some significant reports of a different type. "The long pauses spoil imagery—make it disconnected." "Greater speed resulted in fewer irrelevant images." "In the fast reading imagery was not nearly so pronounced (as in the slow) but more of a unity." Here, as elsewhere, I find testimony that fits in with Dr Olive Wheeler's emphasis of the value of continuity of imagery ('duration' as she calls it) as when, to quote one student, images 'well into' one another. This seems to be characteristic of several of the most facile imagers. But I cannot find that it is essential to the helping of appreciation by imagery. In the various experiments described in this paper a number of the subjects who have only intermittent imagery still claim that it helps.

XII.

I present the following conclusions:

1. It is clear that, especially if the poem involves descriptions of natural objects, a majority of the persons tested experienced imagery and found that it added to their enjoyment even if it be only intermittent. In some cases the imagery seems to be the main source of pleasure.

(2) Very decided individual differences, however, are found. Even the nature poems are highly appreciated by some without any perceptible imagery. This does not bear out the suggestion that visual imagery is essential, as it is said to be by some literary critics.

(3) As several most reliable subjects report, imagery which is vague and indefinite may be just as helpful as very definite clear images, though in a somewhat different way. Clear visual imagery seems to make one element of the poetic enjoyment to approximate to the actual enjoyment of beautiful scenery. Vague imagery tends to go with a more reflective attitude.

(4) A great range of capacity for imagery revealed itself. With some imagery is insistent in the reading of poetry. It comes without the slightest effort: it 'cannot be suppressed.' In several cases the same image is recognised as recurring constantly with a poem, and so on. With some, various images move and blend and form a continuous flow. On the other hand some get no imagery even with a nature poem and can get scarcely any even by the greatest effort.

(5) A great capacity for facile imagery is no guarantee that it will be used in the appreciation of poetry. And the same individual may have vivid and enjoyable imagery with a poem at one reading and none at another, though the enjoyment may be as great.

(6) A law of compensation or rivalry is suggested. Visual imagery is reported to displace or be displaced by, auditory imagery, or by emphasized attention to rhythm, sound or meaning. It does not therefore always follow that the addition of imagery to one's experience in reading a poem will ensure additional enjoyment. Nevertheless it would seem that when imagery does occur, the more facile it is, the less likely it is to displace other elements. It may 'run on' as a tune runs on in the head without displacing other processes appreciably.

(7) Though imageless thought about a beautiful object (*e.g.* natural scenery or a beloved friend) may be tinged with strong feeling, the definite addition of feeling when an image is revived and the predominance of imagery as inspiration of the creative work of the poet, indicate that normally one may expect the immediate emotion associated with a beautiful object, to be best revived only when it is imaged.

(8) The functioning of imagery in poetic appreciation cannot be fully expressed in the statement that the words of the poem produce images which carry with them feeling. The following also seems to happen: words of the poem stimulate feeling, which rouses imagery, which may rouse further feeling.

(9) This rousing of imagery by feeling is the best safeguard against irrelevant imagery which may spoil appreciation. Imagery may be of objects not strictly included in the content of the poem, and yet may not spoil appreciation if it is congruent in feeling.

(10) Dwelling on imagery, even only to the extent of observing it while reading poetry may result in irrelevant imagery, or in revelant imagery being emphasized unduly. But on the whole, most individuals seem to gain by having their attention called to the possibilities of the enjoyment of imagery in certain kinds of poetry. This applies more especially to those who have a fairly facile capacity to image, but who have not used it reading poetry.

(11) While a deliberate attempt to image, by dividing attention and over-emphasizing imagery, usually spoils the appreciation of a poem, a process of self-coaching in imagery often tends, with suitable poems, to increase the enjoyment of the poem through enriching imagery.

(12) Such deliberate attempts, even in their after-effects, are, however, sometimes only harmful or at least partly harmful, and this suggests

that the method should be used sparingly in the teaching of poetry in schools. The safest plan would seem to be the general cultivation of visual and auditory imagery (or the prevention of its loss through neglect) and the leaving thereafter of the arousal of appropriate imagery to the feeling stimulated by the poem.

(13) Much, however, depends upon the nature of the poem. A purely descriptive nature poem, for example, is a fairly safe poem in which to encourage imagery. Some peculiar personal associations may spoil the effect, but this is less likely if feeling is genuinely roused, and feeling is more likely to be roused if imagery is active. This assumes for its full effect, however, that the individuals concerned have had direct experience of the natural objects described—which does not apply in the case of many town children.

When, however, a poem is full of metaphor or symbolism, there is much greater danger of any encouragement of imagery over-emphasizing the symbol. In the nature of things, while a word may readily stand for two different things (in virtue of its symbolic function) and blend the feeling of both, the image is nearer to the concrete thing. It is possible, however, that those individuals to whom visual symbols appeal strongly (as in religious ritual) may find such imagery in poetry less disturbing. When a metaphor is dangerously weak (as in Thompson's *Hound of Heaven*), a visual image may greatly spoil the effect.

(14) The images connected with past emotional experiences which revive when elements of a favourite poem are dwelt upon, probably indicate some of the causes of the strong feeling of the poem. The strong feeling attached to some poems certainly seems more comprehensible sometimes on the assumption of such normally unconscious factors. Such associated images may come up in the ordinary reading of the poem. The extent to which they are congruous with the feeling of the poem determines the extent to which they help (or hinder) the appreciation of the poem perhaps even more than does their similarity of content.

NOTE. Since this paper has been in type, I have had the opportunity of discussing with Dr Olive Wheeler the experiments described in her article mentioned above. She tells me that the judgments given in the table on p. 236 of her article, refer to the third reading of the poems, but that the introspections given on p. 236 were written after some supplementary questions had been put, and that she doubts whether in these reports, the writers separated clearly the second and third readings; they reported rather on the whole experience.

THE SOCIAL AND GEOGRAPHICAL DISTRIBUTION OF INTELLIGENCE IN NORTHUMBERLAND

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1. Social Distribution.

On February 24th, 1922, an Intelligence Test[1] was taken by all the children attending elementary schools in Northumberland (excluding Newcastle and Tynemouth) who were over 11 years and under 13 years of age on the 1st day of March following. It was also taken by most secondary school pupils of those ages. Results were obtained from 13,220 children in elementary schools, and 405 children in secondary schools. In view of the large numbers concerned the results have been grouped under eight classes, instead of giving the exact Intelligence Quotient for each child. These classes were as follows:

Class	I.Q. of	Number of Girls	Boys
A+	=140 or over	4	12
A	=130 to 139	49	80
B+	=120 „ 129	318	414
B	=110 „ 119	1146	1129
C+	=100 „ 109	1765	1833
C−	= 90 „ 99	1779	1757
D	= 80 „ 89	991	981
D−	=Below 80	622	715
	Spoiled papers, etc.	21	9
		6695	6930

Teachers were asked to state the occupation of every child's parent; but in 176 cases, this information was either not given at all or was obscure. Omitting these, as well as the 30 spoiled papers, there remain 13,419 children, who are here grouped together according to their parent's occupation, the average Intelligence Quotient of each class being shown.

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[1] No. 2 Northumberland Mental Test (Harraps), somewhat similar to that described in this *Journal*, XII, Pt 3, Dec. 1921, but rather more difficult. The norms from which the Intelligence Quotients given in the present paper were calculated are given in the *Teachers' Manual*. They were based mainly on the Northumberland results themselves. The test papers were marked by a team of my students under the direction of my colleagues, Miss S. Bailes and Mr Vernon Brown, to whom my sincere thanks are due. Mr Duff is responsible for the analysis into occupations. G. H. T.

The relation established between occupation and intelligence is of course not direct; the occupation is the parent's, the intelligence is the child's; and 'intelligence' must only be taken to mean those particular qualities which were measured by this particular Intelligence Test.

As far as possible, the occupations have been grouped to show differences of social standing, of skill and of responsibility. For this reason, the Census classifications are not very helpful; they are mainly concerned with the numbers engaged in each industry, such as transport or food or textiles. In the present inquiry it is more important to place a man by his standing in the industry, and therefore more or less by his intelligence. But it has not been possible to do this accurately; the information at hand is not definite enough. For instance, the word 'engineer' may include many different ranks of skill. It will be seen too that many groups contain extremely few cases, and naturally some of the smallest groups are those of highest social standing. It would be difficult to draw a line at the exact number of cases which constitutes a representative group; so the numbers are given as they stand. The average Intelligence Quotient has been reckoned by taking each class letter at its middle value, *e.g.*, C + = 105, C − = 95. This will probably raise the average somewhat above the truth, since the lowest mark thus obtainable is 75, whereas the highest is 145. But of the 13,419 included, only 16 children reached A + (= 145), and 128 reached A (= 135). On the other hand, it is to be hoped that not many children with an Intelligence Quotient much below 70 are to be found in ordinary schools. The average Intelligence Quotient thus reckoned for all the 13,419 children is 99·6. The average Intelligence Quotient of each group is given in the following table[1]:

	Occupation	No. of cases	Average I.Q.
A[2]	Clergymen	13	121·2
A	Lawyers	4	120·0
A	Teachers	40	116·0
E	Chemists	4	115·0
C	Bankers	15	111·7
B	Managers (miscellaneous)	63	111·5
A	Artists, musicians, writers, actors	17	110·9
E	Dealers in dress (miscellaneous)	11	110·5
C	Clerks, secretaries, draughtsmen	160	110·2
A	Higher officials, govt. and local govt.	38	110·0
C	Commercial travellers	78	110·0
I	Patternmakers	14	110·0
	Ships' officers, pilots	21	109·8
C	Merchants and agents (miscellaneous)	45	109·7
A	Surveyors, architects, civil engineers	9	109·4

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[1] We shall be glad to communicate the distribution of each group to anyone interested.

[2] The letters in this column show which occupations are combined in the larger groups given in Diagram 1.

	Occupation	No. of cases	Average I.Q.
C	Telegraphists	12	109·2
F	Ships' engineers	10	109·0
J	Coach-, wagon-, wheel-makers	6	108·3
J	Dyers	3	108·3
J	Paviors	3	108·3
F	Colliery engineers	28	107 9
E	Innkeepers	74	107·7
D	Policemen, warders	56	107·3
E	Cabinet-makers, undertakers	26	107·3
E	Drapers	42	107·1
J	Printers	19	107·1
G	Foremen (miscellaneous)	56	106·8
B	Colliery managers, coal merchants	29	106·7
E	Grocers, fruiterers	116	106·0
E	Saddlers	11	105·9
	Railway guards, railway inspectors	30	105·7
D	Soldiers, sailors (R.N.), coast-guards	27	105·4
E	Butchers, fishmongers	105	105·0
J	Domestic servants	11	105·0
C	Insurance agents	58	105·0
I	Makers and repairers of motors and cycles	18	105 0
J	Millwrights	9	105·0
J	Soap and candle makers	2	105·0
F	Electricians	61	104·8
E	Barbers	20	104·5
E	Dairymen	15	104·3
E	General shopkeepers and assistants	69	104·1
E	Bakers and confectioners	51	104·0
J	Textile workers, rope-makers	9	103 9
E	Tobacconists	8	103·7
F	Engineers (unspecified)	202	103·5
I	Tin and brass workers	24	103·3
D	Postmen	46	103·3
E	Boarding-house and restaurant keepers	17	103·2
E	Nurserymen	11	103·2
E	Storekeepers	22	103·2
H	Carpenters and joiners	305	103·1
M	Dock labourers, trimmers, teemers	69	103·1
E	Shoemakers	47	103·1
J	Chemical workers	20	103·0
J	Sawyers and coopers	39	102·9
E	Tailors	62	102·9
E	Dealers in machines and instruments	9	102·8
L	Gardeners and estate workers	121	102·8
G	Colliery foremen, deputies, check-weighmen	146	102·7
J	Millers	13	102·7
H	Painters, decorators, glaziers	89	102·5
H	Plumbers and gasfitters	63	102·5
F	Chauffeurs and car-drivers	68	102·4
L	Woodcutters and foresters	48	102·2
	Station-masters	14	102·1
H	Builders	40	102·0
	Retired, pensioners, independent	30	102·0
A	Doctors, dentists, vets.	16	101·9
E	Jewellers, opticians, watchmakers	19	101·8
J	Cement, pottery and glass-makers	12	101·7
J	Paper-makers	6	101·7
E	Book-sellers and newsagents	9	101·7
I	Fitters, turners, drillers	119	101·6
J	Packers, furniture removers, warehousemen	11	101·4
I	Blacksmiths	181	101·4

	Occupation	No. of cases	Average I.Q.
F	Railway engine drivers and firemen	202	101·0
I	Ship-building workers (miscellaneous) ...	217	100·8
J	Caretakers, commissionaires, watchmen ...	52	100·2
H	Bricklayers, masons, plasterers	209	100·2
J	Hospital and asylum attendants	16	100 0
L	Gamekeepers and rabbit-catchers	61	99·9
I	Iron and steel workers	142	99·9
	Railwaymen (miscellaneous)	201	99·7
J	Brick-makers	15	99·7
L	Shepherds	96	99·1
	Dead (former occupation not stated) ...	793	99·0
I	Boiler-makers	105	98·9
J	Carriers, carters, vanmen	171	98·7
K	Quarrymen	70	98·6
J	Barmen	25	98·6
G, L	Farm-stewards, farm-foremen	54	98·5
K	Colliery workers (miscellaneous)	231	98·4
L	Farmers	319	98·4
K	Coal miners, hewers	5659	97·6
M	Corporation workmen, scavengers, sextons ...	129	97·6
J	Gas workers	18	97·2
M	Building trade labourers	5	97·0
M	Labourers (unspecified)	570	96·2
H	Tilers, slaters, roofers	11	95·9
	Seamen, ships' stewards, bargemen, fishermen	120	95·8
M	Washerwomen and charwomen	24	95·4
L	Coachmen and grooms	36	95·0
K	Clayminers and leadminers	8	95·0
L, M	Farm labourers...	382	94·3
J	Brewery and mineral-water workers... ...	12	94·2
	Cripples and invalids	19	93·9
L	Cattlemen	13	92·7
M	Hawkers and chimneysweeps...	35	91·0
	Insane, criminal	7	87·9

If occupations more or less similar in standing are grouped together, the following result is obtained:

		I.Q. <100		I.Q. =100			I.Q. >100				
		D −	D	C −	C +	B	B +	A	A +	Total	Av. I.Q.
A	Professional	4	6	10	34	42	33	7	1	137	112·2
B	Managers	3	5	13	20	32	12	6	1	92	110·0
C	Higher commercial class	13	16	56	96	108	62	14	3	368	109·3
D	Army, navy, police, postmen	3	10	30	42	28	11	5	0	129	105·5
E	Shopkeeping class ...	27	74	164	202	187	77	14	3	748	105·0
F	Engineers	45	58	117	173	119	49	7	3	571	102·9
G	Foremen	10	31	56	87	60	7	5	0	256	102·7
H	Building trades ...	49	75	196	195	140	52	9	1	717	102·0
I	Metal workers, ship-builders	61	94	223	235	160	40	7	0	820	100·9
J	Miscellaneous industrial workers	40	57	122	145	76	26	5	1	472	100·6
K	Miners and quarrymen excluding managers, foremen, etc.	684	1028	1662	1494	866	206	25	3	5968	97·6
L	Agriculture, all classes	143	193	313	264	149	53	13	0	1128	97·6
M	Low grade occupations, labourers	171	239	336	291	128	44	5	0	1214	96·0

This result may be expressed diagramatically thus[1]:

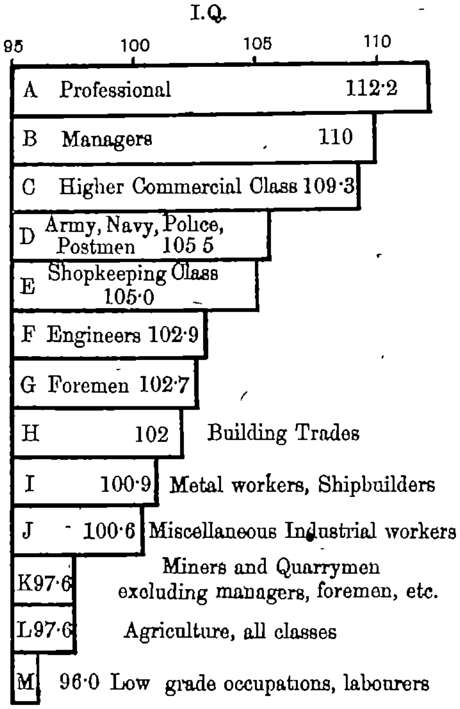

Diagram 1.

Lastly, an attempt has been made to place the children in two divisions—(*a*) those whose parents' occupation is writing, teaching, giving orders or selling; and (*b*) those whose parents' occupation is making, mending, moving, growing or tending any kind of goods. 'Brain-work' and 'hand-work' are convenient, if inexact, names for the two divisions. 'Brainwork' includes all Groups A, B, C, E, and G; and also ships' officers, police, railway guards and inspectors and station-masters. Four groups have been omitted for want of more certain information, viz. retired, etc., dead, cripples and invalids, and criminals and lunatics. All the remainder have been assigned to 'hand-work.' The result is as follows (see also Diagram 2):

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[1] In spite of the steady change in average I.Q. with social standing as indicated by occupation, there is little accuracy in prediction from occupation to I.Q. in individual cases, and much overlapping. Thus, when L (Agriculture) is omitted as being partly included elsewhere, and the columns A and A + are combined to avoid zeros, the 'correlation' by Pearson's first contingency method is 0·28.

Brain-work 1,722 children, average I.Q. = 106·6
Hand-work 10,848 „ „ „ = 98·6

I.Q. = 100

	D −	D	C −	C +	B	B +	A	A +	
Brain	61	140	321	478	459	204	51	8	1,722
Hand	1160	1700	2964	2815	1655	475	71	8	10,848

2. GEOGRAPHICAL DISTRIBUTION.

The conclusion which one of us drew tentatively in 1921 about the geographical distribution of Intelligence Quotients was as follows: "The distribution of intelligence suggested by the tests is such that the highest ability appears to be found *close to* the cities and *far away* from the cities, the intermediate areas having fewer cases of high ability, as though they were drained by selection." It was added that this must be accepted "only with the greatest caution." (This *Journal*, 1921, XII, 222.)

This year there has been much better opportunity of comparing districts since *all* the children of certain ages were presented for the test. The figures for *elementary* schools are as follows:

District (elementary schools only).	Cases	Frequency of classes A + and A	Percentage of cases over I.Q. of 99
1. Tyneside from Hexham inclusive to the sea, including Gosforth and Whitley, but excluding Newcastle and Tynemouth	4431	1 in 96	48
Whitley and Monkseaton	(395)	1 in 30	67
Gosforth	(324)	1 in 108	57
2. Seacoast, excluding Blyth, Whitley and Monkseaton, and Berwick	707	1 in 101	42
Blyth	1173	1 in 147	53
Berwick	380	1 in 95	52
3. Coalfield, defined as the district south of the Coquet, and between the 250-foot contour line and the sea, excluding the areas given above	4614	1 in 231	48
4. The rest of the county, mainly composed of the Belford and Ottercap moors, the valleys of the Till, upper Coquet, Rede, N. Tyne, S. Tyne, Upper Derwent	1798	1 in 95	49
Total	13,103*		

* Certain schools are omitted from this table for various reasons.

Bearing in mind the possible sampling errors of these numbers, it will be seen that although a superiority of the towns is evident, it is only well marked in Monkseaton and (to a less extent) Gosforth. Gosforth is a residential suburb close to Newcastle, Monkseaton another such district, on the coast a few miles away, with an express electric service to town.

Monkseaton elementary schools have, especially in certain cases, a very high reputation[1].

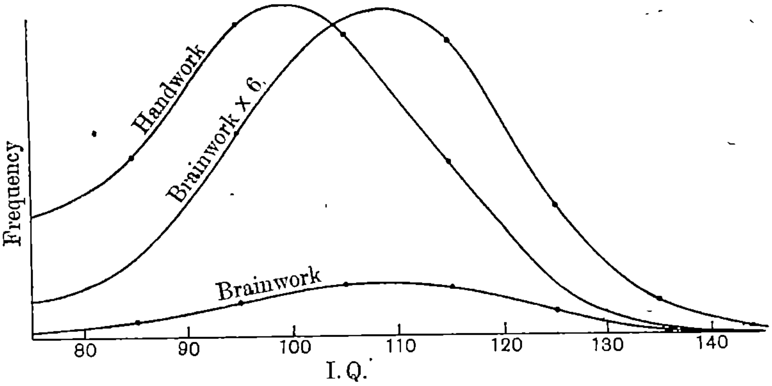

Diagram 2.

The coalfield is low in the frequency of classes A and A +; and both in this and in the average the most rural region (4) is quite as good as the whole (mainly urban) district of Tyneside, excluding Newcastle. Looking at the numbers broadly, they still seem to lend some support to the hypothesis that the superiority of the residential suburbs is due to selection, by the operation of the social ladder, from the surrounding districts: and that this has not affected the more remote districts so much as the coalfield.

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[1] Because of this, and the difficulty of travelling, probably a number of children attend elementary schools in Monkseaton whose parents would send them to private schools if they lived in Gosforth or Newcastle.

(*Manuscript received*, 12 *June*, 1923.)

THE PHILOSOPHY OF HARMONISM IN ITS PSYCHOLOGICAL BEARINGS AND FUTURE EXPERIMENTS[1].

By Sir CHARLES WALSTON.

I cannot flatter myself that every member of this audience has read my recent book[2] in which, in a compressed form, I have given the main principles of the philosophy of Harmonism, to be elaborated in further detail in the future, so far as time may be spared to me to accomplish this task. Nor can I dwell to-day on those aspects not directly psychological, such as the system of aesthetics itself in all its branches, which of course forms an important factor in the philosophy of Harmonism.

For psychology the central truth of the philosophy of Harmonism lies in the fact that, beyond the special differentiation of the several departments of accurate human knowledge, science or philosophy, a 'harmonistic principle' is to be found as the primary factor (*a*) in the objective world outside of man's sentient and cognitive faculties or receptivities, *as well as* (*b*) in the development of his organs and activities of sentience and thought and, above all, (*c*) in the relationship between the outer and inner world underlying all sentience, perception and epistemological activities. As a recent American reviewer of my book[3] has admirably put it in a compressed form:

> Harmonism is manifest in the laws of nature. In the forms of symmetry, order, periodicity, rhythm, it appears in the inorganic world. It is likewise evident in the lower forms of life. (Loeb and Jennings are cited in evidence.) Without harmony between stimulus and reacting organ no reaction occurs in living things. From the lowest forms of man's physiological activities to the highest forms of justice, harmony is to be found. There is no department of life or no form of man's activity which does not display it.

In brief, I maintain that the process of all sentience, sense perception and cognition depends not on a *monistic* principle (either mechanistic or vitalistic), but on the *triadic* principle of Harmonism. This triadic principle manifests itself in three aspects or facets:

(1) anatomically as well as physiologically in the sensory organs

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[1] An abstract of a paper read before a General Meeting of the British Psychological Society, June 2nd, 1923.

[2] *Harmonism and Conscious Evolution* (John Murray, London, 1922).

[3] Prof. Adam Leroy Jones in the *New York Evening Post* (Lit. Rev.), May 12, 1923.

and their functioning, in which their symmetrical nature is a dominant factor;

(2) physically (in outer nature) in which the so-called 'laws of nature,' their activities and relativities are symmetrical or manifest some form of harmonism;

(3) that, in order that any form of sentience may be induced, the sensory organs and their outer stimulation must be so 'attuned' (I borrow the term by analogy from wireless telegraphy) that they harmonize with the outer stimulus.

Let me but recall the fact that we (and lower organisms as well) are always surrounded by millions of series of stimuli, while only one series at a time reaches our sentience—not to say consciousness. That one series depends upon the favourable conditions implied by the phrase: 'the triadic principle of Harmonism.'

I have deliberately chosen the term Harmonism to bear this wide and heavy weight of meaning, because I know of no term which can contain and imply in some form the aesthetic element in the simplest, even mathematical, relationships of the inorganic and organic world without, and the elements of the aesthetic, differing from, and sometimes even opposed to, the purely mechanical, covering even the highest forms of scientific and artistic receptivity and production in man's conscious existence. I have deliberately discarded the term 'beauty' from this phase of the inquiry, as being both too complex and too anthropomorphic, as well as the terms 'form,' 'order,' 'orderliness,' 'regularity,' 'proportion,' 'symmetry,' 'rhythm,' etc., for similar reasons, which I need not develop here. I may, however, add that the terms 'symmetry' and 'rhythm,' established by the Greeks long ago with very definite meaning and application, serve at an early stage of our inquiry a very useful purpose in their application to the simplest objects and processes in nature and in the anatomy and physiology of man, as well as to the higher forms of art, both in the contiguity of space and in the succession of time. But 'harmony,' closely associated from the beginning with music in the Greek use of the term, is equally applicable to the relation of numbers, forms in space and the harmonious relationships in the most complicated phases of life. It covers with definiteness all this infinite range of meaning.

I have further tried to establish the general idea of force, tendency, impulse or instinct, as occurring in nature, in biological forms (plants as well as animals) and in man, by the term 'harmoniotropism.' But this active factor of harmoniotropism, when, in the ascending scale, we reach the intelligence and directly willed activity of man, merges into another force distinguished by the term 'aristotropism.' These two concepts merge into one another by infinite gradations and shadings of meaning in all sentient beings, from pure reflex actions to conscious volition and design. The less conscious and willed the activity (whether physical and outwardly manifest, or within the inner processes of thought), the more does aristotropism merge into harmoniotropism, though the latter in its widest significance includes all such activities. When, however, aristotropism manifests itself in man, it leads to the more or less conscious perception of a perfectly and fully harmonious image of the action willed or aimed at in its fullness and completeness. In the production of such images, memory and association (themselves essentially based on identity or similarity, which are forms of harmony) are the active factors. This constitutes the essential nature of what I consider in this sense to be the meaning and essence of 'imagination.' In this highest form aristotropism chiefly manifests itself, in the first instance, in the *choice between one or more alternatives*. But in every case, whether it leads to the right or the wrong results in the eyes of subsequent judgments, it does act at the time, in the choosing and deciding will, as the *best* alternative. In every phase, however, there is always an aesthetic residuum in the perception, the state or mood, the choice and the decision of the acting subject. Harmony is the ultimately deciding factor.

This, in short, is an outline of the leading features in the philosophy of Harmonism. But it is needless for me to say that there are innumerable points of great importance which demand further elaboration, confirmation and even experiment. Among these points, in the verification of which I am now engaged, and which will directly interest those engaged in psychological and physiological inquiry, I would single out two or three which may be of interest to you.

The first problem requiring elaboration concerns the relation between Harmonism and truth. In the chapter of my book dealing with this question I have, in the first place, drawn a distinction between pure and applied science. Pure science, to my mind, is reducible to mathematics and logic, in which numbers and words are the respective units or factors. It is the harmonious relation between each truth and the receptive human mind or intellect that leads to *conviction*. So, too, the applied sciences establish a relation between things and events in outer nature and the further relation and meaning which these bear to the human mind and to human life.

That the pure sciences of mathematics and logic depend ultimately on such harmonious relation when they prove to be true, will hardly need further confirmation or emphasis. But they may produce what I call 'conviction,' namely, the *emotion* corresponding to truth, and I maintain that this emotion is likewise purely harmonistic, and also aesthetic in character. When, for instance, an arithmetical or geometrical demonstration penetrates, be it through the eye or ear, to what we may call the 'formal surface' of our intellect, it may produce in us much the same effect as in play is produced by the solution of a Chinese puzzle. The same applies to formal logic. When the truth evokes in us what I call 'conviction,' it must further produce an *emotional state* corresponding to this harmonistic relationship established by the mathematical or logical result. And I maintain that this primary state of conviction is essentially aesthetic in character and that it corresponds in nature to the aesthetic emotion evoked by a pure work of art. I go further and maintain that the emotional impulse, as well as its satisfaction, which moves the scientific inquirer to research and discovery, is primary—that it is essentially the same as the impulse of the productive artist. As regards the reader, the same effect is produced by a well-reasoned treatise by Charles Darwin, as is aroused in him by a play of Shakespeare; or—perhaps to a still greater degree on the more formal side—fundamentally the same aesthetic effect is produced by the full appreciation of some work of pure mathematics or logic, on the one hand, as by a fugue of Bach or even a symphony of Beethoven or an overture of Wagner, on the other.

This bold—to some, paradoxical—proposition requires much more proof, investigation and even experiment to be fully confirmed, especially in the simplest fundamental aspects of these two distinct emotions corresponding, on the one hand, to harmony of formal beauty and, on the other, to truth. For if this can be shown to be the case by direct demonstration and experiment, its final application to the so-called laws of nature and of the applied, natural and humanistic sciences, is a much easier task.

The experiments which I am initiating at present and which I hope to carry to some definite conclusion, are destined to decide whether the simplest fundamental forms of harmonistic or aesthetic emotion are of the same nature as, if not identical with, the emotions corresponding to the appreciation of truth.

In a rough and ready manner I have already for a considerable time carried on such experiments, perhaps even to a sufficient degree to encourage me in the main lines of my generalisations as already published. I have, for instance, studied carefully the facial expression of innumerable people when they have gazed on symmetrical bodies, as contrasted with strongly asymmetrical ones; and I have noted the response to symmetry as a primary and direct response to harmony, whereas asymmetrical bodies produced a distinct, if not opposite, expression. In the same way I have noted, especially among the young, their expressions when a simple mathematical problem 'came out right or wrong,' and I believe that these latter expressions are of the same nature as those evoked by symmetry and asymmetry on the aesthetic side.

But I hope that it may be possible to demonstrate by more accurate psycho-physical experiments whether or not this belief is really valid. On the pathological side, Dr Burt and Dr Edgell have drawn my attention to some experiments on feeble-minded children, which have shown that they are defective in appreciating the proper spatial relations in objects—especially written or printed words. A wide and important field of inquiry is here opened out when these phenomena are considered in the light of the Harmonistic theory. When, not long ago, I witnessed some experiments by means of the psycho-galvanic reflex for measuring, or at least indicating, emotional reactions, I suggested that such experiments should be carried further if possible, so as to indicate not only quantitative differences in intensity or in duration, but the qualitative differences in the emotions as well. I am fully aware of the difficulty of such a problem; indeed I realise that it may be impossible of achievement.

At the same time I am organizing experiments so as to record the facial expressions of particular individuals, as well as of larger groups of children, as they respond, on the one hand, to simple aesthetic experiences from objects clearly beautiful or ugly in form and meaning; and, on the other hand, to the presentation of truthful or untruthful results of simple problems. These changes of expression are to be recorded by means of a cinematograph apparatus[1]. Care will be taken to exclude expressions corresponding to heterogeneous emotions of a more complex order, *e.g.* forms or faces evoking associations of a known order (not aesthetic) or startling by their novelty or unfamiliarity—or unbeautiful impressions which appeal to the sense of the ludicrous or evoke disapproval, horror, etc.

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[1] I must insist upon the importance which a vastly systematised system of experiment may have on psychological studies. We are now at the very beginning of such investigations which may in the future approach in accuracy that of the tabulation and comparison of finger-prints. When, moreover, expressions are not left merely to the subjective recording of the observer, but are available in large series of photographs as well as cinematographic records the objective evidential value of careful study and comparison may be incalculable. Facial expressions, gestures, walks, etc. may then be tabulated from large numbers of records rendering accurate information concerning emotions, emotional states, etc.

In my book I have drawn attention to the importance of symmetrical relationship within those sense organs through which our aesthetic impressions are chiefly received—the so-called higher senses of sight and hearing. These I have called the 'dualistic senses.' There can be no doubt that in their activities the symmetrical co-operation of both organs, both eyes and both ears, is of essential importance to produce a just and perfect perceptive experience which may lead to an aesthetic emotion. Thus, when the refraction of the two eyes is imperfect or unequal, the resulting astigmatism is apt to prevent complete or normal vision. The same also applies to the ears. Apart, therefore, from the harmonistic or symmetrical relationship of objects, so that they may produce corresponding impressions in the senses and mind, the harmonistic arrangement and attunement of the sense organs themselves is also of essential importance.

Moreover, what we call the lower senses—the general sensibility of the body (not to mention the two arms and legs) as well as the organs of smell and taste (though not as clearly and essentially bound up in a dualism requiring harmonious arrangement)—are nevertheless dependent upon a symmetrical division in space (front and back, upper and lower, right and left) which is also subject to this primarily harmonistic principle. Indeed the two nostrils have already in their two-ness been the subject of careful physiological inquiry.

And as regards taste, the work of W. Sternberg on *Geschmack und Chemismus*[1] has shown that definite chemical bodies taste either sweet or bitter and that this difference itself depends upon what he calls the *harmony* of the molecular structure of the chemical bodies themselves. If then it is shown that in outer nature—and moreover the most elemental forms—the response to our sense-perception is based upon a fundamental harmonistic principle, and moreover, induces an almost aesthetic response of fundamental character (sweet or bitter) in taste, I need not indicate of what far-reaching importance this fact is as a confirmation of the theory of Harmonism.

Most important experimental work has yet to be carried out to discover whether it is possible to record, by means of instantaneous photographs, the facial expression of infants at the earliest possible age corresponding to clearly aesthetic odours or perfumes, on the one hand, and to stenches, on the other, and similarly to sweet and bitter tastes. Of course care will have to be taken to exclude, even at this early phase of sentience, any associative qualities through memory or instinct, such as the smell of the mother's breast, etc., which may intervene and counteract the pure aesthetic impression of odour and taste; and to exclude such disturbing conditions as arise from variations in the intensity of the stimuli.

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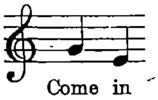
[1] *Arch. f. Anat. u. Physiol.* (1899), 367 ff. My attention has been called to this work by Dr C. S. Myers and Professor F. G. Hopkins.

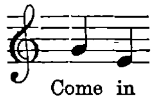
THE CORRELATION OF POETRY WITH MUSIC[1]

By KATHARINE M. WILSON.

PERHAPS it will some day be possible to express the whole universe in one formula. The sciences grow closer and closer to each other, and much of the future of scientific thought seems to lie in relating them, in discovering what relation chemistry bears to animal and vegetable growth, what are the links between physiology and psychology, and so forth. Along this path to one universal formula the arts also must be related; this is perhaps *the* problem for modern aesthetics.

One way of relating things is to look for common origins; the theory of evolution led to the discovery of the link between zoology and botany. So in relating poetry to music we are helped by the fact of their common origin. They were once difficult to differentiate. We need not go back to the close connexion between them in ancient Greek theory and practice, or in primitive peoples of our own era. Many of our folk singers cannot distinguish between the words and the music of their song: the music is as intimate a part of the poem as its rhythm.

If we press the point a little farther, it becomes difficult to distinguish not only music from poetry, but speech from music. Is 

speech or song? Street cries too, are on the border line; and there is good reason to suppose that plainsong also had its origin in the inflexions with which the Latin psalms were recited. 'Come in' is speech because its purpose is utilitarian not aesthetic. But if we make this our distinction, then bugle calls are speech not music. Yet a bugle call may be incorporated in a musical work and become music. The aesthetic quality that turns a bugle call or a musical sound like 

into music, turns speech into poetry. We can therefore neglect it in discussing the difference between music and poetry.

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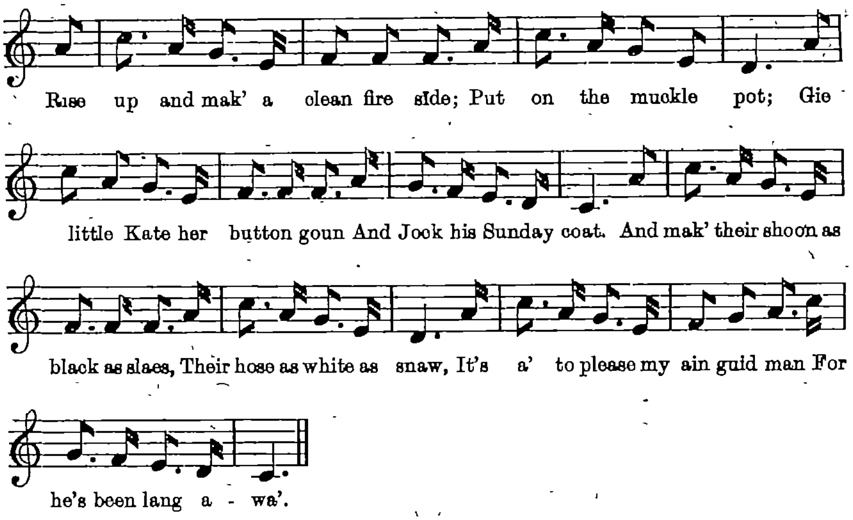
[1] Abstracted from a paper read before the Aesthetics Section of the British Psychological Society, 27 April, 1923.

The chief difficulty of correlating poetry with music lies not so much in any intrinsic difference in these arts themselves, as in the prejudice of poetic prosodists. Though most musicians can read poetry, few writers on poetic prosody can read music. We might think it would go without saying that no one is competent to discuss the relationship of the two arts, who knows only one. But if we assumed this, we should have to neglect half the investigators. The chief argument against us seems to be this: poetry and music are obviously different, so how can they be the same? a symphony is totally unlike a poem. But so is a drum entirely distinct from a flute, and a fiddle is not more like an organ. Music written for a flute cannot be played by a drum, nor can organ music be played on a fiddle. Yet all these instruments are musical, and all the forms of music they produce are musical. Now the voice is the instrument both of song and of poetry. A song sounds more like a poem than like a symphony. Probably the object of primitive song was to give pleasure to the singer. The part of a round that delights us most is the part we sing ourselves. And I think it most likely that rounds—probably our first part songs—and even the earliest madrigals may have been invented, not so much to delight an audience, as to delight the singers. A round and, in a lesser degree, a madrigal are like a poem repeated by several people, each one beginning before the others leave off. *Our* delight in listening to madrigals is a derived one; thus madrigal form seems to us unconnected with poetry, though in the beginning it was probably derived directly from poetic form. The first orchestral music was merely voice parts played on instruments. The later history of instrumental music shows musicians experimenting to solve the difficult problem of how to write music without the support of words. The symphony is the most complex answer. Its form has been evolved expressly to stand without words. That symphonic form is different from poetic form goes without saying. It is almost deliberately different. The symphonic is one of the forms of music farthest from poetry. The form nearest to poetry is that of the unaccompanied song. When we compare poetry with music we compare it, not with a symphony, but with vocal music, or with a tune. We must discover the points of resemblance between a poem read silently or recited, and that poem sung. I am afraid my illustration must be in Scots.

[Here the speaker read the following poem in a Scots accent, then sang it, hoping that her inflexion followed the same tune as the song, and that the audience noticed it.]


Rise up and mak' a clean fire side; Put on the muckle pot; Gie
little Kate her button goun And Jock his Sunday coat. And mak' their shoon as
black as slaes, Their hose as white as snaw, It's a' to please my ain guid man For
he's been lang a - wa'.

There is a difference, but not such a difference as to disprove the idea that poetry is very closely related to, is almost one branch of, music. Poetry undoubtedly arose in the arms of music; the first poetry was song. The original germ of both poetry and music was a tune; and poetry has not developed any farther away from that common origin than has the music of a symphony.

The next difficulty lies in their different scripts. We incline to talk as if all were represented in these scripts; and nothing could look less like a poem than a piece of written music. But very little of the sound of a poem is represented on paper. There are no symbols to indicate either the rhythms or the inflexions. The written music represents little more. Its symbols give us the pitch of the notes, the accents and the time values, and may go some way to indicate the rate, the loudness or softness, and the phrasing. Yet let anyone who can read music examine an orchestral score of something he has never heard, and he will soon discover how little of the actual music appears on paper; as much is left in the hands of the interpreter or to the imagination of the silent reader, as in poetry.

It is no good comparing music and poetry in a vague, general sort of way. In order to compare them in more detail, we must reduce them to the same terms. We must translate either musical symbols into the symbols we use for poetic scansion, or the symbols of poetic scansion into musical ones. When we come to do this, we discover, if indeed we did not see it before, that the symbols of poetic prosody represent nothing at all: they have no definite meaning. – ′ ⌣ may refer to length, or accent, or difference in pitch, or the quality of the vowels, according to our theory, or in some systems indiscriminately. The symbols of music have definite meanings, and mean the same thing independently of all theory. Therefore it seems better to substitute for the vague symbols of poetic scansion, the definite symbols[1] of music; then, at least we shall know what we mean.

I can see no real objection to scanning poetry with these symbols. I think all the objections raised against this method result from misunderstandings. The bar line has caused most trouble, since it suggests a sort of division. Those unaccustomed to musical notation tend to think of the bar line as dividing off the feet, which was never its function in music. Thus Sidney Lanier, though possibly the best prosodian who has adopted musical notation, uses the bar line to mark not the accents, but the feet divisions. He believed that the equal bar, or 'isochronous interval,' is the determining factor in rhythm; that rhythm is all a matter of an equal beat, or pulse. The identification of this theory with the use of musical notation has done the comparison with music much harm. The study of poetic prosody convinces most of us that an equal pulse, or an equal bar is not essential to rhythm—that rhythm depends on something else. The prosodian who believes this, and identifies the 'isochronous interval' theory with the musical system of scanning, naturally rejects that system. But the adoption of musical symbols does not tie us down to any theory; since these signs were evolved to register facts and have no theory attached to them. When, however, we do scan poetry by musical symbols, we find that many of the rhythms of poetry are identical with those of music. The scansion of the typical octosyllabic quartet is exactly right for the typical 16 bar sentence in music, the commonest musical rhythm. And just as poetry may be rhythmic though the bars are unequal, so music has its free rhythms, as we see in plainsong, folk songs, Elizabethan, and in some modern music, where the bars are not necessarily equal.

Some maintain that in English poetry time has nothing to do with rhythm. But their opinion is perhaps cancelled out by those others who hold that time has everything to do with English rhythms and accent nothing at all. If either stress or time were irrelevant, then the comparison with music would be weakened since music has both, though it can dispense with stress, as in organ music. Prosodists who hold views like these may mean one of two things—either that there is no stress or no

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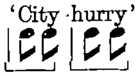
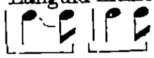
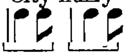
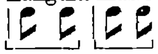
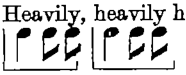
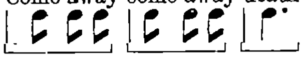
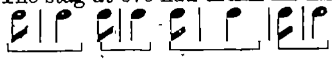
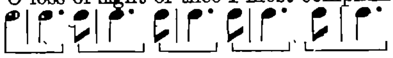
[1] ♩ = 'long' with regard to time; 𝅘𝅥𝅯 = ½♪, which = ¼♩, which = ⅛𝅗𝅥; |♩ = accented 'long.'

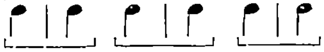
calculable time kept in English poetry, or that stress and time do obtain, but are immaterial to the rhythm. The first position can be upset by experiment. Measurements of phonograph records show that English poetry has both stress and more or less calculable time relations. To exclude time or stress from our prosody is at least dangerous, as any theory based on assumed exclusions must be. We get the impression from such theories that all is a matter of reason, and logic will finally prove one of the partisans right. But I think it time that we stopped talking as if *everything* connected with the arts were a matter of abstract philosophic speculation. All the arts have some basis of fact. Prosody deals specifically with the fact portion of poetry, and it has no right wilfully to neglect any fact. Besides, it does make a difference whether the syllables of a trochee are of different lengths or not.  where all the syllables are 'shorts' or quavers, is different from  where the first syllables are 'longs' or crotchets. I do not mean to be dogmatic about how we ought to read poetry. We might read  so, or  so. What I want to point out is, that the time in which we read poetry does make a difference to the rhythm, and that this difference can best be represented by musical symbols. Give me your reading, and I can give you the symbols. Similarly we may read  with the first syllable 'long,' or  with all the syllables 'short.' And not only having 'long' or 'short' syllables makes a difference to rhythm, the degree of 'longness' or 'shortness' counts too.



has a different rhythm from

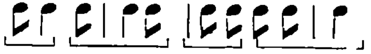


both being different from



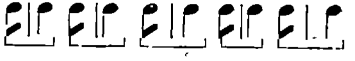
Stress counts too, in rhythm.

'Upon the heavy middle of the night'



has a different rhythm read like this, from

'Upon the heavy middle of the night'



read like this. By stressing 'Upon' and 'of' and lengthening a syllable in 'middle,' and 'of,' we have changed the rhythm. Let me repeat that I do not pretend to say which reading is right (the second may be the right one), merely that they are different, that we cannot tamper with either the natural time or the natural stress of English poetry, without altering the rhythm; consequently that both are important to rhythm.

Another difficulty arises. Many readers of poetry cannot distinguish between stress and length. They will say that the second syllable of 'upon' is not longer than the first or is stressed, or that the first syllable of 'middle' is longer than the second, apparently confusing stress with length. I do not know how far this is prejudice. If we begin to admit the distinction and all that it implies, then many old theories must be given up. The supporters of such theories are most obstinately unable to detect the difference. I fancy we all tend to associate the two: greater length gives us a fictitious impression of stress, and stress a fictitious impression of length. But we can be trained to discriminate between them; and all prosodists ought, in my opinion, to have some such training before they start analysing the rhythms of poetry, that is to say they should have some musical training. A slight experience in music makes a marked difference to the analytic powers of the prosodian. We do not realise how easy it is to mistake time and stress relations till we first try to translate music as we hear it, into musical symbols.

But the most vital obstacle brought against using such symbols is due to a mistake about music. As Mr Omond[1] puts it: "It will also be evident how futile it is to expect correspondence between the methods of metre and music. Musical notes are almost pure symbols. In theory at least, and no doubt substantially in practice, they can be divided with mathematical accuracy—into fractions of $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, etc. and the ideal of music is absolute accordance with time. Verse has other materials and another ideal. Its words are concrete things not readily carved to such exact pattern....The perfection of music lies in absolute accordance with time, that of verse in continual slight departures from time. This is why no musical representations of verse ever seem satisfactory. They assume regularity where none exists. They show syllables as uniform which are really various, and pretend that these keep perfect time when its imperfection forms part of the charm." He takes musical symbols too literally. The whole point of music also, consists in continual slight departures from strict time. The symbols do not represent the real time of music any more than they represent the real time of poetic rhythms. They are only rough indications and were never meant to be anything more. Much work might be done to discover whether poetry or music keeps strictest time. I do not think there is much to choose between. A little has been done to discover how far poetry deviates and how far music. From a rough comparison of Mr Sears'[1] results for music with Mr Warner Brown's[2] for poetry and Dr Scripture's[3] for "Who killed Cock Robin," it seems as if sometimes music keeps stricter time, sometimes poetry. At all events, musical symbols can represent the rhythms of poetry as accurately as they can those of music—that is to say, not very accurately.

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[1] *A Study of Metre*, p. 59.

The interesting fact about the successful fitting of poetry and music —unsuccessful settings are not relevant here—is that both the music and the poetry remain rhythmic each in its own natural way. Poetry written to fit music, does not read as if its rhythms were really musical rhythms not suitable to poetry; nor does music written to fit poetry, feel as if its rhythms were not native to music. Rhythmic poetry makes rhythmic music; rhythmic music makes rhythmic poetry. The rhythmic principles of both seem to be the same. They have the same sort of feet, the iamb being favourite (for English at least), balance in the same way, and have a similar way of phrasing. Some of the simplest musical rhythms and the simplest poetic rhythms are exactly alike.

But besides rhythm, poetry and music have variations in pitch in common, making melody or tune in music, and inflexion in poetry. I have already said that we should have defined the relation between poetry and music, when we discovered the precise relation between a poem sung and that poem recited. The rhythms are the same, but when we sing the poem we give up something in exchange for the tune. We give up the inflexion. The difference between an inflexion like [the speaker then said the words "For he's been lang awa'" with the inflexion given on page 208] and the sung notes, is that whereas in the sung statement we jump the intervening pitches and rest on each note, in the inflected one, we slide through all the intervening pitches, never resting on even the delimiting notes. The intervals taken can be the same. The inflexions of poetry have hardly been studied at all. Indeed we are so little apt to notice them, that many readers of poetry seem puzzled by the suggestion that they form a most important, possibly *the* most important part of the music of poetry. This is not surprising when we come to think of it. A very striking analogy may be found in the *Journal of the Folk Song Society*. Mr Freeman[1] writes about Irish folk-singers, "They enjoy the singing; but how far are they conscious of any of the notes or tunes?...The words are the beginning, middle and end of a song. However minutely a song may be discussed, only the vaguest references to the tune will be heard. The tune is an elusive essence, the mysterious soul of the words. It has no independent existence. It is a part of the singer's secret, indistinguishable from his voice....

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[1] "The Psychology of Rhythm," *Amer. J. of Psychol.* XIII.

[2] *Time in English Verse Rhythm.*

[3] *Elements of Experimental Phonetics*, 547–550.

"Only experience in the country and long conversations about songs will bring you to this conclusion: that the people hardly know anything at all about their tunes. Certainly to-night, as we listen to our second singer, we do not feel inclined to believe it; for he is a great exponent of the local art, tuneful, precise, masterly. You would think that he had studied every effect with professional care; it will be a great shock to you, when you know him better, to find that he is largely unconscious of music. Yet so he is. If you tell him that two of his songs have the same tune, he will answer that that is impossible, since they are different songs. If you then say that the tunes are very much alike, he will agree, and look upon you as a musical genius for having noticed it. 'What a marvellous thing' he will exclaim 'for a man who was not brought up in Irish to know so much about our songs'." This seems almost incredible to us, yet other collectors tell the same story. The analogy is close. Its tune stands to folk poetry exactly where its inflexion stands to poetry, and many readers are as little conscious of the inflexion as the folk singer and his audience are of their tune.

Besides the rhythm and the sound of the words, we hear another music in poetry. Writers talk of the *cadence* of, say, the *Faery Queen* stanza. Many believe this cadence to be merely a matter of rhythm, but is there not some other music in the *Faery Queen* stanza besides the sound of the words and the rhythm? We must remember that rhythm is not sound but movement: it is merely a matter of stress and time. The sort of sound that marks a rhythm is not, strictly speaking, part of the rhythm. It goes without saying that we read poetry with *some* sort of inflexion; and if you listen, not to the words but to the inflexions of poetry, you will hear that they are not prose inflexions—that, in fact, the inflexion is a tune. I am not a good reader of poetry, so I do not know whether I can make this evident, but if you listen very carefully not to the words, nor for the meaning, but to the inflexions, perhaps you may make out a tune from my attempt. [The speaker here read a passage from *The Ancient Mariner*, then dropped into the opening of *Pride and Prejudice*, and finally returned to *The Ancient Mariner* without pausing between.]

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[1] p. xxiv of No. 23, vol. vi (January, 1920); I have his kind permission to quote.

I expect that even if you were Eskimos and did not know Coleridge from Jane Austen, you could still point to the precise spot where I fell into prose and the precise spot where I returned to the poem. The changed rhythm helped of course. But, may I take it for granted that you would have known by the difference in the inflexion? The inflexion of *The Ancient Mariner* forms a tune, and is as distinct from prose in this respect as music is.

I think it is a good argument that when we find poetry written out as prose we read it with prose inflexions. It is an old trick of the sceptic to trip us up by writing poetry as prose; and when he takes us in by it, he thinks he has scored one against us. He may have scored one against the prosodian, or against some prosodians, but not against the correlator of music and poetry. I suppose the arrangement of poems in lines has set many of us questioning. The rhymes perhaps suggest it, but a linear arrangement seems even more essential to unrhymed poetry, for the lines make a difference to our inflexion. Writers of *vers libre* know this, or ought to. The poet laureate in a recent article[1] seems conscious of it, though he tells only of the difference it makes to the rhythm, and though his argument contains a glaring fallacy. I shall read you a little passage from one of Campion's poems, first as if it were written in prose, then as poetry, and if you listen again, to the inflexion, you will hear the difference. "Follow, follow, though with mischief armed, like whirlwind now she flies thee."

Follow, follow,
Though with mischief
Armed, like whirlwind
Now she flies thee.

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[1] In the *London Mercury*.

The distinction between prose and poetic inflexions is so clear, that we almost deliberately make a difference when we see words written in lines.

We may ask where the tune comes from, what or who makes it. The regular rhythm, the lines and other things in the poem make the tune *for us*; but what determines the rhythm and the lines and those other things for *the poet*? It seems an inevitable conclusion that the poet originates his tune, his rhythms and lines being written to represent it. In fact poets are really musicians. They too invent melodies and fit words to them, or write words to a half-conscious melody. A poet musician like Wagner, or a musician poet like Campion, probably invented many of their melodies and the words of their songs simultaneously. I think the impulse behind the musician's art and the impulse behind the poet's can be very like.

We have discussed all the sound material of poetry except the words. Music, perhaps, has nothing to parallel these. Just as the symphony has little to do with poetry, so words have little to do with music, because they are the sounds we have evolved precisely to express ourselves without music. Yet in spite of music's pride in achieving independence of words, even in the most absolute of her forms we can detect the relationhip with inflexion and with the methods of this music of speech. And so likewise, although words are our emancipation from the thrall of music, they too, still bear the mark of their thraldom.

Every written word is a sign representing a sound, but many oral words are exclamations of feeling rather than symbols for ideas. Many words refer to things, especially in prose. But in poetry the words may represent nothing at all; they may be merely sounds. The 'Hey-nonny-nonny' of a song has no meaning. Most of the refrains in poetry mean nothing so much as music, even where they have a meaning.

> My heart is wasted with my woe,
>   Oriana.
> There is no rest for me below,
>   Oriana.
> When the long dim wolds are ribb'd with snow
> And loud the Norland whirlwinds blow,
>   Oriana,
> Alone I wander to and fro,
>   Oriana.

'Oriana' has no real meaning here. It is an unnecessary vocative, not there to let us know he is still addressing the same lady, but for its sound. Indeed a refrain or repetition may weaken the meaning though it strengthens the poetry. Thus,

And wilt thou leave me thus,
for shame,
To save thee from the blame
Of all my grief and grame

is more expressive as mere meaning than

And wilt thou leave me thus?
Say nay, say nay, for shame,
To save thee from the blame
Of all my grief and grame,
And wilt thou leave me thus?
Say nay! say nay!

It is not only in refrains that words are used more for their sound than their sense. When Shelley writes

The tumult of thy mighty harmonies
Will take from both a deep autumnal tone.

we cannot help feeling that his words were chosen for the sound, and we appreciate them more for their 'autumnal tone' than for their meaning. What does 'vast' mean in

In the dead vast and middle of the night?

Many brains have sought the exact meaning, and I should not be surprised if some more clever than Shakespeare, had amended the line to make the sense clearer; but Shakespeare is wiser in the sound. He often uses words in this way, sounding well, but meaning nothing definite. And even when the words in poetry mean something almost as definite as they do in prose, they are yet chosen and arranged with a feeling for their music. The sound of poetry is always as important as the sense. The poets use words quite as much for their sound as for their sense. Too much can be made of the definiteness of meaning in poetry as opposed to music. Even where poetry paints us pictures, its pictures are not necessarily more definite or more universally the same for every reader than the pictures painted by music.

It is a beauteous evening, calm and free,
The holy time is quiet as a Nun
Breathless with adoration; the broad sun
Is sinking down in its tranquility;
The gentleness of heaven broods o'er the sea.

I do not suppose that anyone sees exactly the same picture as I do; indeed, I see a different picture each time I read it. Nor can we say what picture Wordsworth saw, or if he saw any. Does he give you the picture of a nun kneeling in the dim twilight with upturned face? Is she in grey or black? Does he give you evening as we meet it along the cliffs when the tide is out, or is it the deep quiet that broods among the hills? Perhaps he gives you two pictures, nun and evening, or perhaps no picture at all. Can we really say that the meaning of poetry is more definite than that of music? I am not sure that there is not an 'absolute' poetry as there is an 'absolute' music; that is to say, a music or poetry meaning something so definite that we cannot translate it into any other sound without changing the meaning, and yet so indefinite that we cannot tell what it means.

I feel conscious of having treated this huge problem in a very cursory and inadequate way. It is impossible in so short a time to point out all the correspondences, and to answer all the objections to the correlation of poetry with music. Neglecting the more purely psychological comparison, that between the inspiration or the method of poets and musicians, and that between the reader of poetry and the listener to music, I have tried to show that the rhythms of music and of poetry can be identical, that the tunes of poetry differ from the tunes of music in being inflected instead of sung (I sometimes think even this difference disappears in intensely lovely poetry), that just as music has developed forms like the symphony, not strictly comparable with the forms of poetry, so poetry makes use of words which in their aspect as mere labels have developed away from music, whereas poetry differs from prose in using the musical sounds we call words as much for their music as for their meaning.

(*Manuscript received* 25 *May*, 1923.)

NOTE ON 'HIERARCHICAL ORDER' AMONG CORRELATION COEFFICIENTS

BY G. H. THOMSON.

IN two recent notes on his "Theory of Two Factors" Professor Spearman has considered some of the doubts which I have raised concerning the validity of his arguments. One point I would like to answer at once. Professor Spearman has found some arithmetical blunders, which I much regret, in the second example which I give in my paper in *Biometrika* XII, 361, and repeat in *The Essentials of Mental Measurement* (Brown and Thomson) on page 178.

I have now twice repeated those calculations and very carefully tested them by checking devices. And since the result of my error (though small) was to make them more or less inconclusive at 20 subjects, I have extended them, by throwing more dice, to 30 subjects, and have carried out an independent experiment and calculation for 60 subjects.

As the original paper was published in *Biometrika*, I am asking permission to publish the details of the correction and extension in that Journal. But since Professor Spearman's criticism has appeared in these pages, perhaps I may be allowed to say at once that the previous conclusions are quite supported by the new results.

PUBLICATIONS RECENTLY RECEIVED

Colour and Methods of Colour Reproduction. By Dr L. C. MARTIN, *with chapters on Colour Printing and Colour Photography.* By WILLIAM GAMBLE. London: Blackie and Sons, Ltd. 1923. Pp. xiii + 187. Price 12*s.* 6*d.* net.

This interesting book contains three parts. The first part is intended for the general reader and deals with a variety of matters from some discussion of the physical basis of colour experience, to information concerning colouring materials. The discussions in this part are clear, but not particularly well arranged. The second part is rather more advanced. It contains four chapters dealing with photometry, instruments for various colour measurements, and some discussion of the theories of colour vision, and the varieties and tests of colour blindness. The third part is technical, and concerns colour printing and colour photography. The best part of the book is the way in which it indicates to any student large numbers of interesting experiments which he can carry out for himself. The theoretical discussions would be better were they more comprehensive. To the student of experimental psychology the second part will be the most interesting.

Intelligence Tests and School Reorganisation. By L. M. TERMAN, V. E. DICKSON, A. H. SUTHERLAND, R. H. FRANZEN, C. R. TOPPER, and GRACE FERNALD. London: George G. Harrap and Co. Ltd. 1923. Pp. viii + 111. 4*s.* 6*d.* net.

The various chapters of this book were prepared as a report to the American National Education Association on the Revision of Elementary Education. The work is of a preliminary nature and gives an account of a number of investigations that are being carried out on how to adjust school "methods, curricula, and organisation to the individual differences of pupils as shown by mental tests." As helping to suggest lines of practical organisation that ought to follow the application of tests the report is both interesting and valuable.

Introduction to the Use of Standard Tests: A brief Manual in the Use of Tests of both Ability and Achievement in the School Subjects. By Dr SIDNEY L. PRESSEY and Dr LUELLA COLE PRESSEY. London: George G. Harrap and Co. Ltd. 1923. Pp. vi + 263. 6*s.* net

Not everybody recognises that it is more difficult to test than to be tested. There is no doubt that training in the use of mental tests, and a due appreciation alike of their purpose and of their difficulties, are extremely important. This book, which is clear, very well-arranged, and comprehensive should help many students who wish to apply for themselves the tests which current psychological discussions are continually forcing upon their attention.

Matter, Life, Mind, and God. By Professor R. F. ALFRED HOERNLÉ. London: Methuen and Co. Pp. viii + 215. 6*s.* net.

The aim of the author, which is to present a brief outline of what some of the leading thinkers of our day are saying about the problems named in the title, is admirably fulfilled in this volume. Lectures IV and V will be of chief interest to students of psychology, and Mr Hoernlé is to be particularly congratulated on the skill with which he achieves a 'synoptic' view in the domains of psychology and religion. The bibliographies and notes at the end of each lecture are admirably devised to guide those who are unfamiliar with the literature.

Elements in Thought and Emotion. By GEORGE G. CAMPION. University of London Press, 1923. Pp. xvi + 162. 7*s.* 6*d.* net.

Mr Campion calls his book an essay on Education, Epistemology and the Psycho-Neural Problem: there is perhaps more of epistemology than of anything else. The work is well-written, very interesting to read, full of illustrations, not remarkably original, and on the whole of literary rather than of scientific value.

A Study of American Intelligence. By Dr CARL C. BRIGHAM. Foreword by Dr ROBERT M. YERKES. Princeton University Press. London: Humphrey Milford. 1923. Pp. xxv + 210. 16*s.* net.

This book opens with a very appreciative foreword by Dr Yerkes, who describes the occasion and manner of its compilation, and points out its psychological and sociological importance. The treatment is divided into two parts. Part I describes the American Army Tests Alpha and Beta; presents briefly the distributions of intelligence which they indicated; and shows their relative values. There is also a brief discussion of the reliability of the measures obtained. Part II gives a detailed statistical analysis of the results secured by the use of the Army Tests. It deals with these results mainly as they affect the problems of immigration into America. But the discussions are highly significant for this country also. The book is a thoroughly workmanlike production, and well merits the weighty praise given to it by Dr Yerkes.

Dreams. By Dr H. TASMAN LOVELL. Sydney: The Australasian Association of Psychology and Philosophy. 1923. Pp. 73. 2*s.* 6*d.* net.

This short monograph gives first a clear exposition of Freud's theories about dreams, and secondly a number of criticisms of these theories. The criticisms are well expressed although they are not very original. The author, objecting to the treatment of the dream as a "wish fulfilment," urges that it should be treated rather as "a play of values." By "play of values" he means "those uncontrolled changes in feeling which are never matters of express volition, but rather hints at the varying courses taken by our preferences and avoidances." So that, as usual, the objection turns out to be rather to the *term* "wish" than to the notion which the term is intended to express. Dr Lovell's remarks on the ways in which the "dream mechanism" is expressed in normal waking life are much to the point.

Handbuch der Vergleichenden Psychologie: Band I, Abteilung 1: *Tierpsychologie* von GUSTAV KAFKA, S. 1–144; Band I, Abteilung 2: *Psychologie des primitiven Menschen* von RICHARD THURNWALD, S. 145–320. München: Ernst Reinhardt. 1922.

The first two parts of this new handbook of comparative psychology suggest that the whole work will be one of considerable interest and value. Naturally they contain very little that is new, but each gives a good and concise account of a great mass of research, and the bibliographies appended will be found very helpful. Kafka deals with reflex and instinctive reactions; then, much more fully, with various sensory processes; a third, rather short, section discusses perception, and finally a fairly detailed account is provided of various researches in animal learning. Thurnwald deals with practically the whole field of primitive psychology. Perhaps the most interesting sections are those discussing art and writing. The sections on religion and the formation of myths are somewhat scanty. Considering the amount of ground it covers, however, the book is an exceedingly good one.

Psychotechnisches Practikum von Dr FRITZ GIESE. Halle A.D.S.: Wendt and Kluwell, 1923. S. 153.

This is an extremely comprehensive small monograph, in which most of the ground of experimental psychology, particularly in regard to its practical applications, is more or less covered. The whole is put into the form of exercises, with a minimum of discussion and explanatory material, but with some questions attached. In the hands of a competent teacher the book would undoubtedly be useful, but it attempts to deal with too much to be a really satisfactory production.

The Appearance of Mind. By Dr J. C. McKerrow. London: Longmans Green and Co. 1923. Pp. xv + 120. 6s. net.

This is an interesting, vigorous but highly speculative essay in which it is maintained that all life is directed towards what is called "Viable Equilibrium," and that Consciousness "begins with Change of Direction of Movement in Change of Conditions." The notions here expressed are no easier to understand than in many other essays of the same kind.

L'Énigme du Monde et sa solution selon Charles Secrétan. Par Frank Abauzit. Paris: Felix Alcan. 1922. Pp. 235. 12 fr.

NOTES ON RECENT PERIODICALS

Journal de Psychologie, normale et pathologique.

Vol. XVIII. Nos. 8, 9. Oct., Nov. 1921.

This number is devoted to studies in the Psychology of Language. A. Meillet traces the beginnings of language in his *Remarques sur la théorie de la phrase,* from the most rudimentary signs of the transmission of ideas on which he believes the human language to have been built up, these ever becoming more complex with the increase of vocabulary, which was made necessary by the developing intricacy of thought. Meillet analyses the several words in the sentence and shows how the significance of the noun varies according to the amount of inflection it undergoes. J. Vedreyès writes upon the social character of language and the doctrines of F. de Saussure. Charles Bally chooses for his subject one closely allied to the first, *Langage naturel et langage artificiel,* and attempts to trace an historical development in the structure of language. The next paper turns from the psychology of language to that of the stylist *Flaubert.* In a carefully written article, Lips studies Flaubert's indirect method of narration. Albert Sèchehaye writes upon *Locutions et composés,* which he proves are in theory opposed to one another but in practice closely allied. In the article, *Sur la classification psychologique des changements sémantiques,* Leonce Roudet discusses the change in meaning which is found from time to time in familiar words, and compares these investigations with those of Wundt. After this comes a charming paper upon the first attempts at language on the part of children by Oscar Bloch, who considers imitative repetitive sounds as the first step. *La théorie de Helmholtz* is reviewed by H. Marichelle in reference to the question whether the system based upon it is applicable to the teaching of words to deaf-mutes; he is inclined to think the success of the method has yet to be proven, and deplores the fact that the statistics of results are so incomplete that no definite conclusion can be reached. An article translated into French by I. Meyerson, originally written by E. W. Scripture, upon *La parole épileptique,* gives the results of experiments by means of tracings on a drum covered with smoked paper, showing the differences between the vibrations of sound-waves of both epileptics and normal subjects, which however were only to be detected by means of a microscope. *La signification de certains éléments de l'intelligence dans la genèse des troubles aphasiques* is by W. von Woerkom. He holds that in all cases of aphasia a close examination of the evocation of spatial relations and of rhythm is necessary, as these two functions play an essential and primary part in the development of symbolic thought. R. Morgue also gives a summary of *Hughlings Jackson's* method of studying aphasia.

Vol. XVIII. No. 10. Déc. 1921.

L'inspiration prophétique. H. DELACROIX.

We must make a clear distinction between private inspiration and prophetic inspiration, the first being an illumination which lightens the spirit of the person himself, whereas the second is accompanied by the idea of a mission of revelation.

Du rôle des réflexes localisateurs. Le nativitisme réflexe. H. PIÉRON.

This paper discusses the existence of a spatial reflex governing localisation to be found in human beings, analogous to the scratch reflex of the decerebrated dog. Piéron says that it is to be found in the newly born, who withdraw the lower limb upon cutaneous excitation of the plantar surface. Not only does Piéron carry out his experiments with touch sensations, but also attempts to throw some light upon auditory orientation and visual localisation. He asserts that errors in orientation are due to some disturbance of the reflexes. In an appendix to this paper he gives some cases of cerebral lesions caused by shrapnel during the war, with diagrams and details of the autopsy in each case.

Etude expérimentale du schématisme. G.-L. DUPRAT.

From the starting-point that all persons recognise common and familiar objects by the slightest outline or a silhouette representation of them, the writer proceeds to demonstrate that a child's mental images at first take the same form and *pari passu* with the growth of his intelligence more details are progressively filled in.

Vol. XIX. No. 1. Jan. 1922.

Le poids du cerveau, et l'intelligence. LOUIS LAPIEQUE.

This paper deals with the relation of cerebral weight and ocular co-efficient in connexion with intelligence. Occasionally the results seem to tally with what one generally observes concerning the corresponding intelligence of animals; yet, as he himself remarks, it is difficult to believe that the rabbit is 2¼ times more intelligent than the rat. Man, he finds, has a mediocre ocular co-efficient, but his cerebral co-efficient still allows us to estimate his intelligence above that of the other animals.

Religion et nationalité. A. VAN GENNEP.

The author follows other writers on comparative religions in that he agrees that the two are generally synonymous, the great exception being of course the Jews. Van Gennep also discusses the result of changes brought about in nations who have been conquered or converted, so merging a former nationality or religion into another.

Psycho-pathologie de l'indécision. D. WECHSLER.

Asserts that the mechanism of Ambivalence, a fluctuating condition between love and hate, accounts for all conditions of indecision to be found in psychopathic patients.

Some account is also given of the Congress of *Psychotechnique* in Geneva.

Vol. XIX. No. 2. Fév. 1922.

Le langage de la douleur en Chine. GRANET.

Sorrow, asserts the writer, is expressed by means of actions and symbolism, for grief and mourning among the Chinese are silent. The ritual extends over some three years and different symbolic actions are carried out at various stages, although for the whole period the relatives of the deceased are segregated from the rest of their neighbours and to a great extent are not allowed any intimacy one with another. Sexual intercourse is forbidden during the period of mourning. All the chief stimulations of life separate the individual from society. For this reason the author seems to think that the rites, providing a prescribed formula for the expression of private grief, and giving it, as it were, a technical symbolism, are an attempt to unite the individual once more with the group.

Les bases physiologiques de la perception du temps. L. BARD.

Discusses the question of the physiological origin of the perception of time, comparing it with the physiological spatial sense or faculty of localisation. Should children and animals not have precisely the same conception of time as ourselves, they are possessed of an equivalent which serves their purpose equally well.

Critical study of *La psychologie du raisonnement, de L. Eugenio Rignano.* Ph. CHASLIN.

Vol. XIX. No. 3. Mars 1922.

La méthode dans l'étude des dessins d'enfants. LUQUET.

Three methods are described by Luquet. One, the collection of a very large number of drawings of children with the help of collaborators; the second, a study of the drawings of a single child; and the third, that of studying drawings scratched upon walls or hoardings in towns, drawings crude and totally lacking what the others often suffered too much from, suggestion from parents or educators. Of the three, the writer favours the monograph method.

La penseé formelle chez l'enfant. J. Piaget.

This paper describes experiments upon children with tests requiring considerable logical reasoning. Their difficulty seems evident as few of the children tested were able to give the correct solution.

G. Halberstadt also contributes a few notes upon the *Evaluation of Time among Mental Cases.*

Vol. XIX. No. 4. Avril, 1922.

L'écart d'isochronisme dans le rythme. P. Verrier.

Discusses a method of studying rhythm as found in prose, poetry and music in many countries.

Les doctrines indiennes de physiologie mystique. P. Masson-Oursel.

The writer traces the Indian doctrines of mysticism throughout the various stages of the Vedas, the Brahmanas and Upanishads, and the Tantras.

Le renoncement et l'abandon. J. Segond.

Reviews four works by different authors dealing with the psychology of religion. All these works are of an intimate character and are simple biography, but the author considers them to be of the utmost importance for accurate understanding of the development of religious psychology.

Vol. XIX. No. 5. Mai, 1922.

Tendances et faits psychologiques. F. Paulhan.

Considers the question of the Psyche, and suggests a synthetic conception of the Psyche as a more or less systematic ensemble of tendencies, round which would be grouped the various orders of psychological facts, such as sensibility, intelligence, the will, consciousness and unconsciousness. This relation between the central tendencies and the phenomena of psychology is the theme of the paper. In the first part the tendencies themselves are subjected to a close scrutiny; and in the second, the elements, perceptions and images, emotion and volition, together with the manifold expressions which these find in every action of daily life.

Réflexions sur la paramnésie. Déat.

Begins by references to Bergson and his theories of memory. Sometimes it is possible to discern in paramnesia two separate layers, as it were, one superficial and conscious, the other among the deeper layers which finds its expression in the dissociation.

Génèse de la métaphysique. F. Sartiaux.

Vol. XIX. No. 6. Juin, 1922.

Signification et valeur de la psycho-physique. E. Bonventura.

Discusses the views of many psycho-physicists from Weber onwards. The writer expresses himself as anxious to show the supreme importance of psycho-physics for psychology, and to reinstate it in its place of honour from which he feels it has been torn.

L'esthétique fondée sur l'amour. Ch. Lalo.

Holds the view that whereas the modern expression of art is based upon sexuality, the reverse is the case among primitive peoples.

Tendances et faits psychologiques. F. Paulhan.

Tendencies, it is held, become conscious, like so many of our other physiological actions, only when impeded. In a normal state, the tendency in action remains unconscious. This holds good as well for society as for the individual.

Vol. XIX. No 7. Juillet, 1922.

Le rêve chez les primitifs. HALBWACHS.

This writer makes a careful study of the dreams of primitive peoples and their beliefs concerning them, from all the well-known sources, Frazer, Spencer, Durkheim, Tylor, Andrew Lang and others.

Une expression organique de la défense psychique. M.-I. BOAS.

Psychological research by means of the plethysmograph, giving details and graphs of the experiments thus carried out, showing typical and atypical curves.

Among the shorter articles are to be found an interesting paper by André Mayer upon *L'adrénaline et l'émotion*; and *Auto-observation d'une hallucination et d'une illusion* by P. Queroy.

XIX. Année. No. 8. Octobre, 1922.

Quelques mots sur la psychologie de la mathématique pure. PH. CHASLIN.

This is a careful survey of the works of others who have investigated the philosophy of pure mathematics, followed by an investigation of the psychology of mathematics considered as a science operating by means of symbols. It is due to the complication of the symbolic system involved, the author assumes, that the mathematical gift is comparatively rare. Pursued to its widest limits such gift embraces creative imagination, the source of which is unknown to the greatest mathematicians themselves.

Genèse de l'art figuré, I. By G.-H. LUQUET.

An article comparing the first attempts of child and primitive man to represent common objects. The first stage of all, the writer thinks, is usually neglected as it is indefinite; the drawings made by the children often being variously interpreted at different times by the little artists themselves.

Among the records given of pathological cases are to be found that of a female patient at La Salpétrière, entitled *Une Amoureuse de Prêtre*, by J. Séglas and *Notes upon Time Evaluation by a Schizophrenic*, by Vinchon and Monestier; also a critical review by Boll upon modern works upon *Psychology and Relativity*.

XIX Année. No. 9. Novembre, 1922.

La rééducation auditive chez les sourds-muets et chez les autres sourds. By H. MARICHELLE.

A history of the attempt to teach the art of speech to those who are deaf or nearly so, in France. The author points out that other countries were in advance of his own by more than 150 years. The method here described depends upon the degree of deafness, and proceeds upon the assumption that, if sounds can be heard at all, they can be distinguished from one another and reproduced.

Genèse de l'art figuré, II. By G.-H. LUQUET.

This deals with the art of representation as shown by the Palaeolithic remains. The writer believes that again we may see the same process at work as with the children, the drawing of some design more or less at random, or the utilisation of a natural marking on the side of a cave, which would afterwards be elaborated into the likeness of a common object.

Shorter articles in this number comprise notes upon the *Common origin of Language both verbal and musical*, by L. Dauriac, and the *Problem of Art*, by M. Schuwer.

Zeitschrift für Psychologie.

Bd. 89. Heft 1–3. 1922.

Das Intelligenzproblem und die Intelligenzprüfung. Eine Auseinandersetzung mit William Stern. W. PETERS.

This is essentially a criticism of Stern's book *Die Intelligenz der Kinder und Jugendlichen.* Stern is taxed with the neglect of much previous work which results in his one-sided valuation of the work done at Breslau and Hamburg.

Assoziationsgesetz und Geruchsgedächtnis. HANS HENNING.

Smells form associations more easily with feelings, moods and impressions of the lower senses than they do with perceptions and ideas of the higher senses. The reversibility of the associations between smells and other experiences depends upon the possibility of eidetic olfactory images. One single olfactory experience is often enough for retention, because each smell, each different smell intensity, has its own individual single value, so that the various experiences do not enter into associative competition. It is in the nature of the lower senses that their experiences are coloured by feeling-tone which is maintained also with involuntary attention.

Psychologische Untersuchungen an Hühnern. AUGUST RIEKEL.

The most important experimental results of this research are stated to be (1) It could be shown by these investigations on fowls that the process of comparison depends originally upon the effectiveness of "transition-experiences" (Übergangserlebnisse) like those which Jaensch has shown to exist with eidetics, i.e. subjects that possess pronounced memory images. (2) In comparing colours fowls judged especially by their brightness, the colour itself was of secondary importance. (3) Whilst the fowls could very well compare shapes and colour shades, they were incapable of comparing distances. (4) The "transition-experiences" could be demonstrated to exist with fowls even when the objects were no longer given side by side in pairs, but successively. (5) Comparative investigations with children showed that most children between 2 and 4 years of age behaved as fowls did; that is they were practically guided by the transition-experiences. (6) Language can materially influence the formation of judgments. (7) When comparing shapes children judged decidedly much earlier absolutely than when comparing colour shades. This depends probably upon the fact that there are names for magnitudes before there are any for colours. (8) With reference to the question of the "blue-blindness" of fowls it was ascertained that the fowl-eye is sensitive to blue, but that it is much less so than the human eye.

Über den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter. V. *Über Raumverlagerung und die Beziehung von Raumwahrnehmung und Handeln.* E. R. JAENSCH.

The author continues from a previous paper to give an account of his investigations. He is principally concerned with the self-directing processes which he finds play a prominent part also in the psychology of space. The spontaneously occurring space-displacements and their relations to visual space are investigated, the conditions favourable to their occurrence, the relations of optical spatial displacement and mirror writing, spatial displacement of real objects, spatial displacement and wandering of attention, the dynamic components of effective wandering of attention are all discussed.

Next spatial displacement is considered as a splitting up of given complexes and a preparation to thought.

Finally the author speculates "On the significance of action for the building up of spatial perception."

Bd. 89. Heft. 4–6. Leipzig, 1922.

Zur theorie der stroboskopischen Bewegungen. FRANZ HILLEBRAND.

The paper begins in the present number of the *Zeitschrift* and is concluded in Vol. 90, Nos. 1, 2. It gives a historical review of earlier theories of stroboscopic movement and mentions the "theory of unobserved gaps," that of "after-image effect," Wertheimer's "short-circuit theory," Linke's "Theory of assimilative perception" and Koffka's views on "Stroboscopic movement and successive light mixture." Stroboscopic movement is not regarded as the filling up of a gap, but as a revaluation which the place of the second object experiences. That which the author has called "revaluation" does, he says, in fact not deserve this name. Not the function of the retinal elements concerned has become different, but the visual field determined by its boundaries has been shifted. With involuntary eye-movements this "revaluation" does not take place. When a retinal image is transferred from one retinal place to another without a correspondingly large displacement of the visual field then a pseudo-movement is produced.

Über die Erblichkeit der musikalischen Begabung. V. HAECKER and TH. ZIEHEN.

This is a continuation from Nos. 3–5 of volume 88.

Umriss-skizze zu einer theoretischen psychologie. J. LINDWORSKY.

The author objects to metaphysical discussions on the relations of mind and body serving as introduction in textbooks on theoretical and experimental psychology. Nevertheless he accepts in his sketch some form of psychophysical parallelism as a working hypothesis, at least for sense perceptions but rejects as unproven the existence of physiological processes as necessary attendant or conditional phenomena in the case of perceptions of relations and conations.

Bd. 90. Heft 1–2. Leipzig, 1922.

Zur Theorie der stroboskopischen Bewegungen. FRANZ HILLEBRAND.

Zur Psychophysik der Geradheit. EDGAR RUBIN.

To determine definitely whether a given object is straight or not in the physical sense, a physical examination is necessary, but a physical examination does not help in the least to decide whether the object appears straight; and the object of the paper is to ascertain the relations between physical straightness and the appearance of straightness.

Sozial Verhältnisse bei Vögeln. THORLEIF SCHJELDERUP-EBBE.

This is a preliminary note on observations made on many kinds of birds belonging to different classes and genera. It was found that whenever birds of greatly different kinds habitually meet there is always established a social rank-order.

Bd. 90. Heft 3–6. Leipzig, 1922.

Untersuchungen über die sog. Zöllnerschen anorthoskopischen Zerrbilder. HEINRICH ROTHSCHILD.

If certain simple figures pass behind a narrow slit in a screen they appear to be distorted in the direction of their movement. With a high velocity they appear shortened in this direction and with a low velocity lengthened. These are Zollner's anorthoscopic distortion-figures which have been made the object of an experimental investigation by the author. It is found that the phenomenon cannot be explained by eye-movements and all other previous attempts at an explanation are rejected. The author thinks that the difference between the figure and its background as well as the figures themselves and their contours are of the greatest importance and that a psycho-physical mechanism requiring further explanation has to be postulated for the fact that an object passing behind a narrow slit appears broader than the slit itself, and makes this impression immediately and simultaneously.

Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe. JACOB BAPPERT.

The results of this investigation are (1) Impulse to convergence and to accommodation are just as little factors in depth-perception as are the state of convergence and accommodation or the change of this state. (2) The fact must be regarded as proved that of two objects of the same retinal size the objectively nearer one appears more frequently as farther away and smaller. This phenomenon is directly connected with accommodation and convergence without being traceable to an error of judgment. (3) It may happen that the distance of two objects from the observing eye remains quite uncertain whilst the depth-distance between these two objects can be apperceived with certainty. (4) The change in convergence and accommodation with adjustments to very different depths is noticed only in a few cases (excepted are those cases where the object is close to the near-point and can be seen distinctly only with great effort, and even then the perception is often without any certainty as to depth). (5) Neither does spontaneity give a definite depth-value in the change of accommodation and convergence, as was supposed by Hillebrand.

Über die Erblichkeit der musikalischen Begabung. V. HAECKER and TH. ZIEHEN.

The two earlier instalments in this investigation were noted previously; the present one forms the conclusion. The purpose of the investigation, state the authors, was by means of large statistical material to ascertain the conditions of inheritability, the development, the components and the correlative conditions of musical ability.

The authors terminate their paper with this statement: "We repeat that our investigation has also pursued principally methodological aims, and that we harbour still many doubts as to the objective results. We are therefore continuing our research and ask our colleagues most earnestly to furnish us with any actual or literary material." A bibliography is given.

Über den Einfluss der Grössenvariierung bei Gedächtnisleistungen. MARTIN FLOORS.

Black figures or shapes of varying sizes painted on a white ground were used and the methods of recognition and of hits were adopted.

Neue Untersuchungen zu einen Falle von abnormem Datengedächtnis. R. HENNIG.

The author refers to two previous articles on his doubtless quite abnormal memory for dates. It was only in his thirtieth year that he became aware that the remarkable certainty and extent of his memory were unique. In his 49th year, the time of writing, he finds that in spite of increasing years his memory shows not only no impairment but rather a constant increase.

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THE NATURE OF GENERAL INTELLIGENCE AND ABILITY (I)[1]

BY GODFREY H. THOMSON.

THE title chosen for this symposium has the merit of brevity and clearness in directing attention to the matter to be discussed. But like most titles it tends to beg the question, and it strongly suggests that some entity named general ability or intelligence exists, and that its nature can perhaps be ascertained.

That is not, I take it, the real meaning of those who selected the phrase. If it were, they would be committing the 'faculty fallacy.' We may ask why it is that Jones excels in almost anything he turns his mind to. But we may not reply "because he possesses general ability," or we shall be on a par with Molière's physician (whom Dr Stout quotes in this connexion) who explained that opium produces sleep because it has a soporific tendency.

The legitimate suggestion of the title is, that a man who is good at one kind of mental work is usually above the average in others. The question it asks is, why this should be so.

As to the facts implied, the suggestion is, on the whole, correct. In technical language, most measures of correlation between various mental tests, or between various school and university subjects, are positive and many are high. Though some are low, few are negative. When this is denied, it is generally on the strength of a number of individual cases where marked ability is found in one subject but not in another[2].

These are, however, swamped by the much larger number of cases in agreement with the principle. Those who do not handle correlation coefficients daily hardly realise how many and how large discrepancies can coexist with a high correlation coefficient. In a correlation table which lies before me now, showing the performance of 1034 Northumberland school children in two tests, 237 are on different sides of the average in the two tests, and some are far above in one and below in the other. But the correlation is 0·735, and the general fact remains that a child who is above the average in one kind of mental work is *likely* to be above the average in another[1]. Any search for the reason of this must, however, take into account the existence of such exceptions. There is, too, another fact, of which I am less certain, which must be considered: that is, that exceptions become, I think, more numerous as children grow older and develop into men and women. Of this, if it be so, an explanation close at hand is that they develop habits which confine them to certain lines.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

[2] "The public are, in general, very ready to adopt the opinion, that he who has pleased them in one peculiar mode of composition, is, by means of that very talent, rendered incapable of venturing upon other subjects....There is some justice in this opinion, as there always is in such as attain general currency....But much more frequently the same capacity which carries a man to popularity in one department will obtain for him success in another" (Sir Walter Scott, Introduction to *Ivanhoe*, 1830).

The general point of view which I shall take up in what follows is that we shall be wrong in restricting ourselves to finding *one* reason for this general fact of positive correlations: that there are probably a number, possibly a large number, of reasons which are differently operative in different regions of thought: that one man may be good, or bad, all round for quite different reasons from those which make another good or bad: and that one and the same man may do a thing well on one occasion in one way and on another occasion in another way. In short, my thesis is that, although people tend, no doubt (though with many exceptions and inequalities) to be 'generally able,' yet there is no such thing as 'general ability,' no one factor which, by its amount in a man, determines his performances in general, and is the sole source of correlation between them[2].

On the contrary, I prefer to think of the mind as much more complex, and the kind of complexity I mean is very like the kind of complexity found in the brain and nervous system. The latter is composed of units, which, however, are connected and inter-connected in an infinity of ways. I believe it is correct to say that they are more numerous, and their inter-connections yet more infinite, as we ascend the scale of intelligence in the animal kingdom: and most so in man. In the same way the mind, it seems to me, has an innumerable array of responses to situations[3], which responses are inter-connected in a tangle of associations, within which tangle are distinguishable various plans or patterns: distinguishable yet not distinct, as the concealed puzzle picture is distinguishable in the drawing of a landscape.

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[1] Some, however, think this may be an illusion. See, *e.g.* Pressey, *Psychological Review*, 1920, XXVII. 469.

[2] I would beg permission at this point to explain that I wish to keep this paper free from a controversy which I have waged with Prof. Spearman on the question of the validity of certain mathematical methods he has used. Even were Prof. Spearman in the right in that controversy, the general factor might still be complex, as he has himself always said. And even if I am right, the general factor may still exist, as I have always said: though in that case it not only *may* be complex but I think *must* be so. However that may be, I wish here to think it out apart entirely from the mathematical argument based upon the correlation of the correlation coefficients.

[3] "Not, like the sands of the sea, innumerable disconnected atoms, but growing out of roots, clustering in families, connecting and intertwining themselves with all that men have been doing and thinking and feeling from the beginning of the world till now" (Trench, *Study of Words*, 43–4). He is here speaking of words, but the sentence expresses well the complexity I have in mind.

An animal which had only one response to a given situation would not be said to be intelligent. It would try the same response again and again in spite of failure. An animal with many responses to a given situation would seem more intelligent. It would try *a* and fail: then *b* and fail: then *c* and *d* and, finally, succeed. More accurately, it responds to the *new* situation caused by the failure of *a*, the failure of *b*, and so on. It might return to *a* and try it again later, or even quite soon. Very slight differences in the environment or in the set of the organism might alter the order in which different individuals tried their repertory of responses. We should call such conduct 'trial and error' and deny to it any high degree of intelligence.

An animal, however, which had some means of anticipating the success or failure of each response without completely carrying the response out would save a lot of time and would do its trial and error by proxy, as it were. The most obvious source of such anticipation is in imagery of various kinds, which may both be produced by such frustrated responses and may aid in shortening them.

Dr Ward, I think it is in his Encyclopaedia article, propounds the hypothesis that images first arise when a familiar chain of events is interrupted: as when a baby hears a door opened, a footstep, a voice, and then for some reason its mother's caress does not follow. This seems to suggest neurones being set in readiness to respond, and then, on one occasion, responding without further outer stimulus; as we take two pulls to a rifle, and may take the second even though the expected target fails to appear.

If this is so, the animal which realises, without completing it, that the response is useless, has gone ahead further along the path of neurones which are ready to explode. Their state of readiness, *plus* the external situation, is enough to produce an unsatisfying effect, and another response is tried instead. Or in other words, imagination has carried out the response, and has found it to fail. Such an animal is man, who sits down and thinks the situation out. That is, he tries a number of responses by proxy. We all of us feel the temptation to try some of them actually, at any rate by incipient movements. We gesture. We walk towards the place. We pick up the stone and poise it. Children and unintelligent people do so more readily.

The evolutionary process which creates the animal with several possible responses to one situation, or strictly to nearly the same situation, is, it seems to me, very likely at the same time to produce an animal which can and will respond in such and such a way to a number of different situations. If we have six situations with a specific response each, it seems likely that it will be some of these six responses which will be tried one after the other in situation No. 1, in the less 'specific,' more intelligent animal. And this implies the corollary that response No. 1 may be aroused by some other situation than No. 1. It is not, of course, necessary to assume that any situation is *equally* likely to incite this response. Some may not do so at all.

This progress from instinct to intelligence, therefore, seems to me to imply, or to make probable, a 'general ability' by releasing the responses from their specific character and binding them, though in unequal degrees, to a larger number of situations[1].

The most extreme of the 'general factor' theories, on the other hand, namely, that of Prof. Spearman, explains the facts of correlation by postulating energy which flows in from the whole cortex, or from the whole nervous system, and reinforces the particular set of neurones which are immediately associated with the mental activity in question. It has, in Dr McDougall's 'drainage' hypothesis, a strong moral support, but I find a great difficulty in realising exactly what is meant. As long as one keeps strictly to the mental side of the gulf, and speaks of energy in the same way as the Freudians speak of libido, the difficulty is perhaps less. But the energy of a neurone, I understand, is resident within the neurone. A neurone goes off rather like a cartridge or a fuse. It does not conduct like a wire. And under these circumstances I do not know what is meant by energy flowing in. However, another suggestion of Prof. Spearman's is more comprehensible. The blood-stream is, as he says, an obvious common factor, and since he wrote[2] we have come to recognise more fully that the blood-stream carries not only nutriment, but also those stimulating or soothing hormones produced by the glands. "Effective mental ability," says Mr Punnett in his *Mendelism*[3], "is largely a matter of temperament, and this in turn is quite possibly dependent upon the various secretions produced by the different tissues of the body. Similar nervous systems associated with different livers might conceivably result in individuals upon whose mental ability the world would pass a very different judgment. Indeed, it is not at all impossible that a particular form of mental ability may depend for its manifestation, not so much upon an essential difference in the structure of the nervous system, as upon the production by another tissue of some specific poison which causes the nervous system to react in a definite way."

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[1] I am convinced that such a hypothesis can explain the correlational facts, including that degree of correlation between the correlations which actually exists.

[2] *Brit. Journ. of Psychol.* 1912, v. 71.

[3] Macmillan, 1919.

It is in Mendelism, where the idea of units of inheritance has proved so fruitful, that the idea of mental ability being dependent upon a number of unitary factors finds hope and support. These unitary factors, however, need not, as the above passage shows, be exclusively factors involving the quality and structure of the nervous system. Nor are they likely to coincide with any more or less unitary qualities commonly used in classifying the minds with which we are daily brought into contact, such as quickness, or instability (I am quoting loosely from Mr Punnett).

These units, if heritable and traceable, would provide just the kind of explanation of general ability which would most easily fuse with what experience I have had and with what opinions I have formed in the matter. For it seems to me very unlikely that any such inherited unit should not be advantageous in more than one kind of situation. If a response, or some quality of response, favourable in situation *A*, is detrimental in situation *B*, it is very likely to be bred out in favour of some response which does possess the vicarious virtue of being usable elsewhere. The great majority of bonds, I believe, are free on the whole from the defect of being, in some other situation, detrimental. Not all, I know. I have heard it said that cycling is bad for speed in swimming, and probably it is so for the best swimmers and for the time being. But there must surely be, on the whole, a very considerable correlation between speed in cycling and speed in swimming. Similarly, speed and accuracy in arithmetic are correlated, though excessive attention to either can reduce the other.

Not only is it difficult to say what factors might possibly be inherited as units. It is very difficult generally to disentangle the inheritance of mental ability from the influence of the environment. But on the whole I believe inheritance to be much the more potent factor. As an example of the kind of evidence which is being accumulated bearing on the point, I may perhaps quote the case of the siblings whose general intelligence has been measured in California by Dr Kate Gordon (see *Biometrika*, 1923, XIV. 388, in a paper by Miss Ethel M. Elderton). Siblings, that is, brothers and sisters, show a considerable degree of correlation in any measure of general intelligence. It has been put, by good judges of the weight to be attached to the various measurements, at about ·5[1]. This correlation clearly may be due to the environmental influence of the common home. John and Mary may be both good, or both bad, not because they are sib, but because they have enjoyed the same home advantages, or suffered from the same disadvantages.

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[1] Some find a smaller value, *e.g.* Pintner, *Psychological Review*, 1918, XXV. 254.

But Dr Kate Gordon has measured this same correlation in the case of some 200 or more pairs of siblings who have all for many years enjoyed the *same* home advantages, in certain orphan institutions: and she finds it the same. This is strong evidence, and is not unsupported. Most teachers, too, agree that ability is, in the main, inborn: and I am disposed to attach considerable weight to the opinion of teachers, who are persons not without some psychological training, and in constant contact with the young. It may be replied that the whole performance of education implies the contrary opinion. But more and more people, I believe, are coming to look upon education rather as a means of discovering than of creating talent. The 'formal discipline' paradox itself can be resolved by this hypothesis. It is not that Latin, or Mathematics, or any of the 'hard' subjects, *make* a man able by reason of their prolonged mental training. They *find out* that he is able by providing a prolonged mental test—unnecessarily prolonged, if that were their sole purpose.

To return to the animal which I left trying over its repertory of responses by proxy. Imagery, I said, might be one means of doing this. A yet more powerful means is by the use of symbols, especially by the use of language. A map is a symbol of the real country over which a general has to move his troops. He tries movements on the map to ensure the selection of the right movement. The most flexible of all symbols are words, and there is no doubt that the acquisition of vocabulary is closely bound up with general ability. I cannot to-day agree with Montaigne (even though Socrates would have it so) that "he who hath a clear and lively imagination in his mind, may easily produce and utter the same, although it be in Bergamask or Welsh, and if he be dumb, by signs and tokens." Perhaps if he has *become* dumb. But if he is born dumb, he comes to school at seven without a language and without that store of ideas which the normal child has acquired by the aid of language. Not merely without certain information, but without the concept of any except the simplest relations.

It is in this grasping of relations, for which, above all, words or other substitute-symbols are required, that some have seen the essence of general ability, and there is a great deal of truth in their point of view. Among the most successful tests of general ability are those which require it, notably the Ebbinghaus Combination or Missing Word Test, and Mr Burt's Analogies.

Others have seen in the power of attention the central factor (Burt[1], Damm[2] and, in another connection, Groos[3]) and one might well extend this doctrine to include nearly the whole of character. "Sure, he has to have ability," says Judd Jason in a novel[4] of Winston Churchill's, "but his feelings is the driving power; if he feels strong enough, he can make a little ability go a long way." But the feelings, or the attention, are not the ability. They can make it go a long way, but they are distinguishable from it.

But is there anything left after one has in thought removed all the factors which may make ability go a long way? I think that if there is, it is the facility with which old responses can be hitched on to new situations, and the readiness with which a response is discarded, almost before it is begun, if it is going to prove unsuccessful[5]. But it may be that nothing *general* is left at all.

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[1] *Brit. Journ. of Psychol.* 1909, III. 169.

[2] *Beiheft* to *Ztsch. f. angew. Psychol.* 1914 (9).

[3] *The Play of Man*, 1898, Introduction.

[4] *A Far Country.*

[5] Cf. Thurstone, *Journal of Phil., Psychol., and Sc. Method*, 1919, XVI. 561, where he defines intelligence as the remoteness from the overt act at which the reflex circuit becomes conscious.

THE NATURE OF GENERAL INTELLIGENCE AND ABILITY (II)[1]

By ED. CLAPARÈDE.

I.

En proposant à notre Président—qui demandait de lui suggérer des questions à mettre au programme du Congrès—le thème de l'‘intelligence générale,’ j'avoue que je n'avais pas une idée bien nette des contours de cette question. C'est précisément parce que la notion d'intelligence générale m'apparaît comme assez vague que j'ai pensé qu'il pourrait être utile d'en parler entre nous, afin de voir si, au cours de la conversation, nous la verrions se préciser, si nous apercevrions mieux les problèmes qu'elle soulève, et les moyens de les aborder par l'expérience.

Les psychologues savent que les notions, les concepts, sont des créations de l'esprit qui naissent au contact des faits, et qui ont pour fonction de les interpréter. Nous devons donc tout d'abord nous demander quels sont les faits au contact desquels la notion d'‘intelligence générale’ s'est formée.

J'en aperçois cinq ou six.

1. *L'âge mental:* L'observation du développement des enfants montre à l'évidence que la capacité mentale augmente, d'une façon générale, d'âge en âge. Un enfant (normal) de 15 ans, même s'il est parmi les moins intelligents de son âge, est capable cependant de résoudre une foule de problèmes théoriques ou pratiques devant lesquels échouera un enfant de 6 ans, même s'il est très intelligent. Tout naturellement alors nous concluons qu'il existe une sorte d'‘intelligence générale’ qui dépend du développement même de l'esprit, qui dépend de la croissance, de l'âge.

Si nous ignorons quelle est la nature exacte de cette capacité dépendant de l'âge, nous savons cependant très bien ce que nous entendons désigner ici par le terme d'‘intelligence générale.’ Le fait que ce terme désigne n'est lui-même pas équivoque. C'est celui qui est mesuré ou évalué, par les tests de Binet ou les tests analogues. Mais je suis d'avis qu'il vaudrait mieux, dans ce cas, éviter l'expression d'‘intelligence,’ et parler seulement de ‘niveau mental,’ ou mieux encore, d'‘âge mental.’

2. *Différences individuelles.* La psychologie individuelle, comme l'observation courante, nous amène aussi à la notion d'intelligence générale. Nous constatons que certains individus manifestent, d'une

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

façon générale, plus d'intelligence que d'autres placés dans des situations analogues et ayant été soumis à la même éducation. Nous voyons aussi que certains enfants sont nettement arriérés ou avancés sur leurs camarades de même âge. Dans ces cas, cette sorte de capacité intellectuelle dont nous constatons la variation d'une personne à l'autre, n'a rien à faire avec l'âge. Elle constitue une différence individuelle. Chez les enfants arriérés ou précoces, il est vrai, le degré d'intelligence, exprimé par le quotient intellectuel, peut être ramené à un retard ou à une avance du développement mental, et pratiquement nous le traduisons en âge mental. Mais chez les adultes cette réduction ne saurait se faire, et nous sommes enclins à admettre ici que les différences observées tiennent à une différence de degré d'une capacité générale à effectuer des opérations mentales.

Il incombe à la psychologie expérimentale de déterminer quels sont les processus qui sont à la base de cette capacité générale, et si ces processus sont nécessairement de nature intellectuelle, ou si d'autres facteurs comme la volonté, la persévérance, l'intérêt, etc. ne concourent pas au rendement de cette capacité.

3. *Opposition entre aptitudes spéciales et intelligence générale.* Une autre voie encore qui nous conduit à la notion d'intelligence générale est l'opposition que nous constatons entre les diverses aptitudes et la capacité intellectuelle moyenne. Voici deux individus qui semblent également intelligents. Soumis aux tests de Binet ou de Terman, ils fournissent des résultats égaux. Et cependant, ils ne sont pas identiques entre eux; leurs aptitudes ne sont pas les mêmes. Il faut donc admettre qu'à côté de ces aptitudes, il y a chez l'un et chez l'autre quelquechose de semblable, ou d'équivalent. C'est cette chose qu'on appelle leur intelligence générale. Cette intelligence générale se détermine en prenant la moyenne des résultats des diverses aptitudes particulières. Elle est la droite horizontale autour de laquelle oscillent les zig-zags du profil psychologique.

J'ai coutume de donner le nom d''intelligence globale' à cette sorte de capacité générale, qui est obtenue en prenant la moyenne d'un certain nombre d'aptitudes, dont plusieurs sont du reste étrangères à l'intelligence proprement dite, à l'intelligence intégrale (v. plus loin): mémoire, temps de réaction, finesse de la sensibilité, etc. C'est l'intelligence globale que mesurent, par exemple, les tests de Binet ou de Terman. Peut-être vaudrait-il mieux éviter ici le terme d'intelligence, et parler seulement d''aptitude globale.'

4. *Structure des aptitudes et corrélations.* Lorsqu'on calcule les corrélations existant entre diverses fonctions psychologiques, on trouve le

plus souvent un coefficient positif. Ce fait a conduit divers auteurs, notamment Spearman, à admettre que cette corrélation est due à un facteur commun, sous-jacent aux diverses aptitudes particulières, un facteur général, auquel on a donné le nom de 'general factor,' de 'general ability,' de 'facteur central,' et aussi, quelquefois, d'"intelligence générale.'

L'existence de ce facteur central est, on le sait, fortement discutée. Notons ici que, même si l'avenir confirmait le bien fondé de cette hypothèse, ce facteur général ne devrait pas être appelé 'intelligence générale.'

5. *Psychologie professionnelle.* Une autre avenue qui nous amène encore à nous poser la question de l'intelligence générale est celle de la psychologie appliquée, notamment de la psychologie professionnelle. Lorsqu'on fait des examens d'aptitudes, pour l'orientation professionnelle, par exemple, on remarque souvent que les individus qui réussissent le mieux dans les tests d'aptitude spéciale, ou réussissent le mieux dans leur profession, accusent généralement les meilleurs résultats pour les tests d'intelligence. On se trouve ainsi amené de nouveau à opposer l'intelligence générale aux aptitudes spéciales, mais dans un autre sens que dans le cas noté plus haut sous le No. 3. Est-ce parce que leur intelligence générale est supérieure que ces individus manifestent plus d'aptitude à leur métier? Telle est la question qu'on se pose ici. Pratiquement, cela revient à se demander si, dans l'orientation professionnelle, l'examen de l'intelligence générale n'est pas plus utile, au point de vue du pronostic, que celui des aptitudes particulières. Les *army tests*, aux Etats-Unis, ont montré l'importance pratique de cette intelligence générale.

L'intelligence générale que l'on postule dans ces cas n'est pas une notion analogue au facteur central du paragraphe précédent. En effet, on ne regarde pas ici l'intelligence générale comme un *facteur intrinsèque* des aptitudes spéciales, mais comme une capacité existant *à côté* de ces aptitudes, et pouvant, en quelque sorte, les suppléer, ou permettant, grâce à un effort d'attention ou de réflexion, d'en tirer le meilleur parti possible. L'intelligence générale serait ici, vis à vis de l'aptitude, ce qu'est l'intelligence vis à vis d'un outil: elle n'en fait pas partie intégrante, mais permet d'en tirer un bon parti. Dans la théorie de Spearman, au contraire, l'intelligence générale est regardée comme un facteur entrant dans la constitution même de l'aptitude.

6. *L'intelligence intégrale.* J'appelle 'intelligence intégrale' celle qui se manifeste dans les actes véritables d'intelligence, c'est-à-dire dans ceux qui concourent à la solution d'un problème, d'une situation nouvelle. Je montrerai tout à l'heure comment il convient de concevoir cette intel-

ligence intégrale, et nous verrons que l'acte d'intelligence est, dans sa technique fondamentale, toujours semblable à lui-même, quelles que soient les opérations dans lesquelles il intervient. On constate cependant de grandes variétés d'intelligence: les uns ont une intelligence pratique, les autres une intelligence littéraire, d'autres encore une intelligence commerciale, ou mathématique, etc. La question qui se pose est celle de savoir si chacune de ces variétés intellectuelles correspond à une espèce particulière d'intelligence, ou bien si, au-dessous de ces variétés, l'intelligence est *une*, ces diversités n'étant engendrées que par une diversité dans des aptitudes *non-intellectuelles* (diversité d'imagerie, de langage intérieur, de mémoire, d'attention, d'imagination, d'affectivité, etc.). En d'autres termes, l'intelligence intégrale est-elle une capacité *générale*?

(Dans cette hypothèse, les différences quantitatives d'intelligence se ramèneraient à des différences qualitatives dans les opérations à accomplir, ou à des différences dans les aptitudes non-intellectuelles qui concourent à l'exécution de l'opération.)

Théoriquement, ainsi que nous allons le voir, tout acte d'intelligence, qu'il s'applique à un objet ou un autre, se ramène à un schéma toujours le même. Mais cette unité théorique correspond-elle à l'existence d'un facteur unique psycho-physiologique, facteur qui serait l'agent effectif de tout acte d'intelligence, les différences constatées entre les variétés d'intelligence provenant seulement de la différence entre d'autres aptitudes entrant aussi en ligne de compte? La question se pose ici d'une façon très analogue à celle du cas No. 4 (facteur central de Spearman). Il y a cependant entre ces deux cas la différence suivante: D'abord, ici, il ne s'agit que d'aptitudes intellectuelles (impliquant un acte d'intelligence intégrale). Et en second lieu, le facteur central ici supposé est de nature parfaitement déterminée: c'est la capacité d'exécuter les trois temps (que je vais décrire) de l'acte d'intelligence.

On pourrait sans doute indiquer d'autres voies encore qui conduisent à la notion d'intelligence générale (par exemple, la question de la 'culture générale,' du *formal training*; lorsque des pédagogues prétendent que les langues mortes sont indispensables à la culture de l'intelligence, ne préjugent-ils pas, eux aussi, une sorte d'intelligence générale capable de s'appliquer à tout, et qui prendrait son essor au contact de la grammaire latine?). Mais je pense que ces exemples suffisent pour montrer que la notion d'intelligence générale est loin d'être univoque. Tantôt cette intelligence est regardée comme un *facteur* des aptitudes empiriques (cas Nos. 4 et 6), tantôt comme une *résultante* des aptitudes (No. 3), tantôt comme un phénomène *sui generis* (Nos. 1, 2, 5). Tandis que, dans le cas

No. 3, l'intelligence générale est une notion purement abstraite, ne correspondant à aucune réalité extérieure (comme la notion de taille moyenne ou de durée moyenne de la vie), dans les autres cas, au contraire, l'intelligence générale désignerait un phénomène psycho-physiologique concret et autonome.

Notons encore ici deux questions que soulève celle de l'intelligence générale:

(*a*) La question du *degré* de l'intelligence générale. Celle-ci varie, d'un individu à l'autre, sous le rapport quantitatif: on est plus ou moins intelligent, selon qu'on réussit dans des situations plus ou moins difficiles. Or, comment rendre compte de ces différences de degré? Qu'est-ce qui les détermine? Est-ce la force de l'énergie mentale? L'amplitude de l'attention? La rapidité mentale? (Si Paul résout un problème plus vite que Pierre, on le dira plus intelligent.) Est-ce une plus grande finesse du sentiment des relations? Un pouvoir plus aigu d'abstraction? Ou tout cela à la fois?

(*b*) Le rôle que jouent l'*expérience acquise* et l'*exercice* comme facteurs de l'intelligence générale. Quand on parle d'intelligence générale, on fait sans aucun doute allusion à une capacité *naturelle*, *innée*, et qui ne dépend pas des connaissances acquises ni de l'exercice. Mais comment, en pratique, écarter ces facteurs? La richesse du savoir, l'habitude de résoudre des problèmes, sans être en eux-mêmes des phénomènes d'intelligence, interviennent largement dans la manifestation de l'intelligence.

II.

Mais il est impossible de discuter de la question de l'intelligence générale sans s'être auparavant mis d'accord sur ce qu'on appelle l'intelligence, au sens propre du mot—l'*intelligence intégrale*, comme j'ai coutume de l'appeler.

Qu'est-ce que l'intelligence? Pour définir l'intelligence, il faut rechercher ce qu'il y a d'identique dans tous les actes d'intelligence, si différents les uns des autres puissent-ils paraître. Or, ce caractère commun, c'est de *résoudre par la pensée un problème nouveau*. Si le problème n'est pas nouveau, ce n'est pas à l'intelligence que l'individu aura recours, mais à la mémoire, à l'habitude, à l'automatisme. L'intelligence n'intervient évidemment que lorsque l'être se trouve en face d'une situation que ni ses instincts, ni ses automatismes acquis ne lui permettent de résoudre. Et il faut encore, pour que l'acte soit intelligent, que la solution du problème ne soit pas trouvée au hasard, mais par la pensée.

Un acte intelligent est l'équivalent d'un tâtonnement. Il se distingue du tâtonnement des animaux (*trials and errors*) par le fait que le tâtonnement est, ici, guidé par la pensée, notamment par la conscience des relations.

L'intelligence peut revêtir, suivant la direction de son mouvement, deux grandes formes: la *compréhension* (dans laquelle l'esprit va des choses, des sensations, à l'idée qui leur donne un sens), et l'*invention* (dans laquelle l'esprit va de l'idée aux choses qui permettent de la réaliser). Dans chacune de ces formes l'acte d'intelligence comporte trois opérations: une *question*, une *hypothèse*, et la *vérification de l'hypothèse.* C'est l'hypothèse qui est plus spécialement l'équivalent du tâtonnement[1].

L'acte d'intelligence présuppose donc, pour être accompli, le concours de deux fonctions mentales différentes: l'*imagination* (qui suscite l'hypothèse), et les processus encore obscurs qu'on groupe sous le nom de *raison* (qui contrôlent l'hypothèse en la confrontant avec la réalité, ou avec les postulats posés).

Ces deux grandes fonctions, l'imagination et la raison, sont antagonistes, et jouent pour la pensée le même rôle que le ressort et le balancier dans un mouvement d'horlogerie. Le ressort, c'est l'imagination, qui tend à jeter dans l'esprit toutes les idées possibles; le balancier, c'est la raison, qui règle les productions de l'imagination, qui opère sur celles-ci une œuvre de selection. Lorsque la raison est absente, l'imagination déréglée mérite son titre de 'folle du logis.'

Parmi les trois opérations capitales de l'intelligence, question, hypothèse et vérification, la première, la question, ne semble pas indispensable pour constituer l'acte d'intelligence, puisque souvent la question est posée par une autre personne que celle qui la résoudra. D'autre part, le simple fait de se poser une question constitue-t-il un acte d'intelligence? Assurément non, car il y a des questions absurdes. Il est vrai que les questions intelligentes ne sont posées que par des esprits intelligents. Mais si le fait de poser une question intelligente dénote à coup sûr un esprit intelligent, cela ne saurait, d'après notre définition, constituer un acte intégral d'intelligence.

Cette conception de l'intelligence a l'avantage, non seulement de répondre aux faits que révèle l'analyse de tout acte d'intelligence, mais encore de rattacher l'intelligence à un mode d'activité que l'on rencontre chez les animaux les plus inférieurs, l'activité par tâtonnements, dont elle dérive, et constitue seulement un stade plus élevé.

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[1] Cf. pour plus de détails mes articles: "La psychologie de l'intelligence," *Scientia,* Nov. 1917, et "Des diverses catégories de tests mentaux," *Schweizer Archiv f. Neurologie u. Psychiatrie,* 1918, III. 102.

III.

Les considérations qui précèdent, relativement à la nature de l'intelligence, nous montrent que le terme d'"intelligence générale' ne saurait être appliqué qu'à deux choses:

1. A l'intelligence générale intégrale, mentionnée sous le No. 6, conçue comme capacité *fondamentale*, au cas où l'expérience et la théorie confirmeraient son existence.

2. A l'intelligence générale conçue comme capacité d'intelligence intégrale *moyenne*. Pour obtenir ce coefficient d'intelligence générale, il faudrait prendre la moyenne des résultats obtenus avec des tests d'intelligence intégrale seulement.

Peut-on donner à l'intelligence globale le nom d'intelligence générale? L'expérience montre que l'intelligence globale corrélate fortement avec l'intelligence intégrale (Porteus donne comme coefficients, entre le test de Porteus, qui s'adresse à l'intelligence intégrale, et le test de Binet, qui détermine l'intelligence globale, des valeurs allant de 0·60 à 0·70). On pourrait donc, par extension, considérer aussi l'intelligence globale comme représentative de l'intelligence générale. On aurait alors:

$$\text{Intelligence générale moyenne}\begin{cases}\text{Intelligence intégrale moyenne.}\\ \text{Intelligence globale.}\end{cases}$$

En résumé, je propose:

1. De définir l'intelligence vraie, intégrale, comme un processus destiné à *résoudre par la pensée un problème nouveau*—processus de tâtonnement comportant trois opérations: *question, hypothèse, vérification.*

2. De réserver le nom d'intelligence générale à la capacité mentale *moyenne* d'un individu, telle qu'elle ressort, ou bien d'une série d'épreuves s'adressant à l'intelligence intégrale, ou bien d'une série de tests dont plusieurs ne sont pas des tests d'intelligence intégrale (intelligence globale).

3. De ne pas appeler le facteur g de Spearman 'intelligence générale,' afin d'éviter des confusions.

4. De mettre à l'étude les questions posées sous les Nos. 2, 5 et 6, questions sur lesquelles il est impossible de se prononcer à l'heure actuelle.

THE NATURE OF GENERAL INTELLIGENCE AND ABILITY (III)[1]

By L. L. THURSTONE.

I SHALL present for your consideration a brief statement of what I consider to be the fundamental nature of intelligence and the implications of my definition with reference to the other psychological categories. For several years I have been working with intelligence tests and other varieties of mental test and I have often attempted to formulate a definition of intelligence. My definitions have been modified from time to time and I shall present here their present form.

When we define intelligence we start naturally with the products of intelligence. By this I mean that since we can not perceive intelligence or intellect as a tangible thing, we look at the things that are accomplished by means of intelligence in order to frame some hypothesis regarding its nature. One's first attempt is then to say that intelligence is the ability to get along well in society, to succeed in a competitive existence, to keep out of trouble, to be able to take care of one's own affairs with a minimum of guidance. All statements of this kind refer directly to the products of intelligence, but they do not constitute in any sense hypotheses regarding the nature of intelligence itself. Another variant of this kind of definition is to say that intelligence is the ability to learn by trial and error, to profit by experience, to have memory, to reason. All of these definitions are stated so that intelligence becomes the ability to do something and that something is defined. When we describe the products of intelligence we do not necessarily describe what intelligence itself really is. I shall venture a statement regarding the possible intrinsic nature of intelligent action as contrasted with unintelligent action.

I want to show that the degree of intelligence in behaviour can be judged by the degree of incompleteness of the alternatives in the trial and error life of the actor and that the higher cognitive categories constitute incomplete conduct in the process of being formed.

Overt trial and error without foresight is the most unintelligent kind of conduct. If it is a ring puzzle that we want to master we may pick up the rings blindfolded and idly allow every impulse to complete itself overtly. By such fumbling or overt trial and error the rings may come apart without the aid of intelligent guidance. If we want to draw a circle tangent to two given arcs we may shut our eyes and stick the compass into the paper repeatedly and draw circles at random until we are told that the task is done. In overt trial and error we complete all our impulses into overt conduct and we record the failures on the environment, and sometimes also on ourselves.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

Let us now consider perceptual intelligence of the simplest order. The distance receptors give the organism some foresight because the distance percept has as its meaning the equivalent contact experience. If we perceive a puddle in front of us on the sidewalk, the visual percept is the expectation of the contact experience in the puddle, but that contact experience is only partly specified or anticipated in the visual percept. If we were moving by overt trial and error we should complete the impulse and step right into the puddle. By perceptual intelligence we have transferred the trial and error process out of the puddle to the point where we now are. We also move the trial and error process back in time. Psychologically we transfer the trial and error selection from among overtly completed impulses to the stage at which these same impulses are as yet only partly formulated. Perception, or perceptual intelligence, is a device of biological value in that the organism gains some control over future time and space. To live is, in this sense, to telescope time.

If we use perceptual intelligence, or common sense, for the geometrical construction, we shall eliminate many of the trials while they are only partly expressed and before they reach particular form on the paper. Similarly with the ring puzzle we can by looking at it eliminate as foolish many of the fumblings while they are only percepts and before they reach overt and particular form. The psychological basis for this early elimination of failures while the failures are still incomplete and perceptual is the experiential identity of the sense impression and its completed equivalent contact experience. By perceptual intelligence we move the trial and error process from among overt alternatives to the realm of the incomplete and approximate alternatives which constitute perception.

Now let us move the trial and error point still farther in the same direction and we shall come to alternatives that are still more incomplete and loosely specified. The greater part of a percept is imaginal. It is only the insignificant cue of the percept that is sensorial. The percept of the puddle contains a small retinal image and its corresponding sense impression, but the most important part of the percept is its meaning, which is the expectation of getting one's feet wet, of splashing mud on one's self and on others, and possibly of falling into it. If the relatively insignificant sensorial cue be dropped from the percept we have the corresponding idea which is a more tentative form of action than the percept.

An idea is an incomplete expected experience which is accepted or rejected in terms of its potential completion. Ideas are loosely formulated approximate conduct. They differ from percepts mainly in their degree of incompleteness. Suppose that we avoid a certain street because we recall that it is being paved. We take another route, and in so doing we are eliminating a failure before it becomes overtly experienced. The impulse to walk along the bad street is only roughly specified when we recall that the street is torn up. This is ideational intelligence when the trial and error selection takes place with alternatives that have not yet found their stimulus. If we were limited to perceptual intelligence we should walk up to the reconstruction work and eliminate this route perceptually in front of the sign 'street closed.' In doing so we would have specified our impulse to action much more closely than if the same impulse were eliminated while it was still in ideational form before its stimulus was found. Ideational and perceptual intelligence differ in degree only, in their relative remoteness from the determining stimuli, in the degree of completeness of the act at which its overt fulfilment is anticipated. Ideational trial and error is more intelligent than perceptual trial and error. The amount of intelligence is measured by the degree of incompleteness of the alternatives in the effective trial and error life of the actor.

The highest type of intelligence, conceptual intelligence, consists in the capacity to carry on trial and error among the crude, loosely organised and incomplete actions that we know as concepts. A concept is, in fact, expected conduct, so loosely organised, so tentative and incomplete, that our bodily relations in the expected experience are not yet specified or anticipated in the concept moment.

We teach that a concept is the cognitive symbol of that which is common to many experiences. That is true. In order to have the concept 'dog' it is necessary to have seen many dogs. But that concerns only the *derivation* of the concept, which is a secondary matter. It is more important to realise what the concept actually is and does when it is naturally focal. Let us consider an example. When you come to a convention your first purpose is probably to get to the headquarters. As you step from the train into the station, the concept 'headquarters' may be focal in your mind. The concept constitutes an unfinished approximate act. You start to hunt for the stimuli that might by some route particularise your concept into the expected experience. Your trial and error process is at that moment of a higher order than if you were limited to the possibilities of attaining your ends by that only which happened to be perceptually given. In conceptual intelligence we have the trial and error process of purposive conduct transferred to the stage of our impulses at which they are exceedingly vague, at which our bodily attitudes are still to be specified, and at which we have not yet found the stimuli by which our impulses are finally to precipitate into overt experience.

I have outlined what I consider to be the main characteristic of intelligent conduct, the transfer of the trial and error point from overt alternatives to percepts, from percepts to the still more tentative ideas, and from ideas to the still more approximate actions that we know as concepts. This development means that the alternatives in the trial and error process have less and less intension but more and more extension as we proceed from unintelligent conduct to more intelligent conduct. Let us define intelligent conduct as a trial and error process which takes place at that stage of our life impulses at which they constitute incomplete and approximate action. Conceptual intelligence is this same process of verification and selection among alternatives that are not even bodily or spatially defined and that have not yet been particularised by the expected environment.

What I have called our life impulses might also be called conative tendencies, desires, cravings or drives; I think of them as originating in the organism itself. Their complete expression constitutes seen or overt conduct. Their conflicts while partly formed constitute perception and ideation. They become focal at different stages of completion. If the organism can carry on effective trial and error selection among these impulses while they are only partly expressed, it is to that extent intelligent. It has a wider range of possible conduct, it saves time in finding the successful act, and it saves itself from actually experiencing the failures. Abstract thinking is this trial and error process carried on with impulses that are as yet only symbolic of types of experience, unfinished, loosely specified and flexible indices of expected conduct. The impulses probably exist in some form before they become sufficiently defined to constitute thinking. The subconscious is probably the realm of impulses that are not yet sufficiently defined to be cognitive and focal. That person is intelligent whose impulses take cognitive form or meaning while the impulses are still very incomplete. To do that is to think.

Several inferences can be made regarding the construction of tests for the measurement of intelligence if we accept this definition. Obviously those questions would be the best measurement of intelligence which indicate the degree of abstraction of which the subject is capable. Experimental studies of the Binet test questions have indicated that the questions which are relatively abstract for their respective ages are the best measures of intelligence.

When we turn to the non-language tests we find many tasks which require very little abstraction for their solution, and I question whether such tasks can legitimately be called tests of intelligence. It is possible to construct age scales with test material that requires of the subject mechanical manipulation, but unless the scale shows an increasing degree of abstraction as the essential element of the measurement, I should not consider the test to be, strictly speaking, a measure of intelligence itself.

I should differentiate between two types of tests for intelligence. One type of test measures intelligence at work during the test, and the other type of test measures the product of intelligence. If we find that a test of general information places the subjects in the same rank order as an intelligence test, we may assume that the information test measures the product of intelligence which has been at work in the past. To answer information questions in a test does not require intellectual work during the test. On the other hand, a test of ingenuity or a test in which a problem of some degree of abstraction is to be solved is a more direct measure of intelligence, because the test question requires intellectual work and some degree of abstraction at the time the test is being taken.

If we define intelligence in this way, it should be apparent that the training of intelligence can not profitably be limited to particulars. A liberal education consists in the power to strip situations of their immediate, and distracting details, to reduce problematic situations to their universals, and to put them together again better than they were found. The study of particulars, no matter how practical or interesting, does not train intelligence. To educate native intelligence requires drill in the methods of dealing with a great variety of universals. The habit of reducing problems to their universals and solving them with freedom from distracting particulars is the mark of a trained intelligence.

THE CLASSIFICATION OF THE INSTINCTS (I)[1]

BY JAMES DREVER.

THERE is not yet complete agreement as to the sense in which the word 'instinct' should be used. It is therefore necessary to state in the first instance the sense in which it is taken in the present paper. All are agreed that the word should be employed in reference to native modes of reaction, not in their elements, but as behaviour 'forms.' Some psychologists would stop here. Others, however, would hold that these behaviour 'forms,' or reaction patterns, have as their inner or psychical aspect a certain drive, urge, or impulsion, by which alone the observed phenomena can be explained. A third view, which has never found clear and definite expression, but is nevertheless easily traceable in current psychological literature, practically identifies the sense of 'instinct' with the sense of 'emotion.' This third view may be set aside as unscientific and misleading. The second view seems to emphasize the main point from a psychological point of view, and especially with regard to human behaviour. I should hold that it is the impulsion or urge, which in a certain sense—though this must not be misunderstood—controls the 'form' of the behaviour. This aspect of the concrete fact the biologist is only too apt to forget. At the same time it must be recognized that it is equally a mistake on the part of the psychologist to forget or ignore that aspect of the fact which the biologist emphasizes, the behaviour 'form' aspect. The concrete fact has the two aspects, both of which our term must cover. The one aspect may be more objective than the other, since the urge or impulsion can only be directly observed in the introspection of each individual; nevertheless it is not any more real on that account. When we use the term 'instinctive tendency' we emphasize this aspect in particular, but, as I have said, 'instinct' covers both.

The problem of the classification of man's instinctive tendencies is by no means a new problem. Though few among recent psychologists have addressed themselves specifically to the problem, it was familiar enough to the older psychologists from Descartes up to and including Dugald Stewart. In reviewing the older classifications at the present time, there are two main groups of facts or considerations to be kept in view. The one group comprises the facts revealed by the various methods of modern psychopathology, especially the methods of Freud and his school; the other group is related to the development of biological research, and the results achieved. These facts and considerations must, I say, be kept in view, but it by no means follows that the principles underlying the older classifications must in all cases be entirely set aside.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

The principles laid down by Descartes in prefacing his articles on "L'ordre et le denombrement des passions" are in the main biological principles[1]. A modern psychologist—M. Larguier des Bancels—following the same line of thought has recently given us a classification which may be taken as typical of the kind of classification to which this line of thought leads. He divides instinctive tendencies into eight groups: nutritional (les instincts alimentaires), protective (les instincts de défense), curiosity, sexual, parental, social, egoistic, and play[2]. The grouping is different in many respects from that to which Descartes was led, but this is so in the main because the modern writer has adhered to his biological principles of division, whereas the older writer did not, but mingled psychological considerations, and could hardly help doing so in the circumstances and conditions in and under which he wrote. Whether psychological considerations ought to be taken into account, whether it is desirable or useful to take them into account, we shall proceed to enquire presently.

Before considering the simpler and more popular biological classifications of recent psychology, it is worth noting that we may classify instincts on a purely behaviouristic basis. Thorndike[3], for example, has attempted such a classification. It runs as follows: responses of attention, responses of gross bodily control, food-getting responses, protective responses, fighting responses, motherly behaviour, responses to presence of others, responses to approval and scorn of others, mastering and submissive responses, other social instincts. This classification is interesting in many ways. It is exceedingly comprehensive, concrete, objective. Nevertheless of all classifications it seems to me almost the least useful for present-day human problems, and it entirely ignores the work of the psychopathologist.

The popular biological classification of the present day has had its origin in psychopathology, rather than in pure biology. The basis of classification is very obvious. The biological ends subserved by instinctive tendencies appear to fall into three main groups. Hence we have three groups of tendencies—those tendencies whose end is the preservation of the life of the individual, those tendencies whose end is the perpetuation of the species, and those tendencies whose end is the maintenance of the social group, to which the individual belongs. These three groups we may designate for short ego, sex, and herd instincts. The classification is so obvious that it is not surprising to find writer after writer adopting it. Few writers have, however, tried to discuss it as a method of classification, or to develop it. Fortunately that has been done by a recent writer, Dr MacCurdy, and we are indebted to him for bringing the whole question up for discussion.

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[1] *Les Passions de l'Âme*, art. 51.

[2] *Introduction à la psychologie*, chap. VI.

[3] *Educational Psychology*, vol. I.

In his chapter on "Pragmatic Conceptions of Instincts and their Classification[1]," Dr MacCurdy defends this biological classification. In subsequent chapters he develops it by attempting to review the various tendencies coming under each head. Only in the case of 'instinct-motivations' of the ego group, however, is the discussion sufficiently systematic to enable us to tabulate the results. Here we have five tendencies enumerated as belonging to this group—the seeking of bodily pleasure and creature comforts, intellectual curiosity, lust for power, lust for recognition, desire for security, that is, the negative aspect of the ego reaction. The author calls his classification a pragmatic classification, but I am afraid I do not quite understand what he means by this.

Dr MacCurdy does excellent service in emphasizing the fact that our main concern in psychology is with 'instinct-motivation,' to employ his term. It is 'instinct-motivations' that he is seeking to classify. In other words, the main psychological problem being the understanding of animal behaviour and human conduct, any classification of the instincts which ignores this aspect must be considered hopelessly and deplorably inadequate from the psychological point of view. We may accept this view. It is also the only view which can appeal to the psychopathologist. But what is its bearing on the biological classification? Clearly the psychologist cannot rest satisfied with a classification of 'instinct-motivations' or instinctive impulsions on a biological basis, if a classification on the basis of psychological characteristics is possible, and whether it is possible cannot be known until it has been attempted.

In passing, Dr MacCurdy criticizes the efforts at classification made by other psychologists. His criticism of McDougall and Shand seems, if one may venture to say so, to rise out of a complete misunderstanding of the whole psychological situation. "They give us," he says, "long and diffuse catalogues of instincts"—"unfocussed formulations"—and the disputes "degenerate into sterile academic discussions and squabbles about nomenclature." "Their readers are not able to see the wood for the trees." Such criticism is as misleading as it is unfair. McDougall gives no long catalogues of instincts, but a catalogue at least as short as that given by his critic. The meaning and reference of 'unfocussed formulations' and 'sterile academic discussions' may be known to Dr MacCurdy but are far from obvious to the present writer. Inability to see the wood for the trees is indeed a serious deficiency when it is the wood that we desire to observe, but scarcely so serious when our main concern is with the individual trees. Surely if the whole argument regarding a wood is based on the assumption that it is a beech-wood or a fir-wood, it is incumbent on us to examine the individual trees, in order to see how far the assumption is valid. Nor is such an examination a purely academic question if our whole practical attitude towards the wood depends on all the trees being varieties of beech or fir as the case may be. The Freudian sex psychology might be cited in illustration of the point.

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[1] *Problems of Dynamic Psychology*, chap. XVIII. Camb. Univ. Press, 1923.

It is not difficult to criticize destructively Dr MacCurdy's own method of procedure. "If," he says, "instinctive reactions could be separated into groups which could be shown *a priori* to be inherently antagonistic and if clinical experience demonstrated certain abnormal reactions to be definitely related to the preponderance of one group over others, then this grouping would have high pragmatic value[1]." Surely this *a priori* method of approach is not the right one. That is academic, if you like. The inevitable result of such procedure is an attempt to force the facts to fit our theories, a practice that has been rather too frequent in psychology at all times. Perhaps the best recent illustration is Freud's *Group Psychology*. The fact is, the simple biological classification into herd, sex, and ego instincts is altogether too alluring. It appears to reduce so beautifully the complexities presented by human conduct, normal no less than abnormal. This appearance of simplification is a veritable *ignis fatuus*. Theory must start from the facts, and it must return to the facts, interpreting and illumining them.

The three-fold biological classification will not take us very far towards the understanding of instinctive impulsions or 'instinct-motivations.' No biological classification will, if it is a psychological understanding we seek. Is it possible to find a basis for classification in psychological characteristics? The present writer has elsewhere[2] indicated three psychological characters, with reference to which it is possible to classify. These are: (1) relative specificity of evoking stimulus and resulting response, (2) origination and termination of impulsion within the sphere of the affective or without—within the organism or without, in simpler but less accurate terms, (3) relation to the type of experience and response we call emotional. Let us consider each of these briefly in turn.

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[1] *Op. cit.* p. 268.

[2] *Instinct in Man* and *Psychology of Education.*

1. *Relative Specificity.* This is the least important of the three, and the least psychological, if we may put it so. Consider the impulsion to play, and the impulsion to fight. Whether we look at the stimulus aspect or the response aspect, it is obvious that the specificity of the fighting instinct is much higher than that of the play instinct. Only situations having a certain specific character can evoke the first; only in specific forms of response does the impulsion manifest itself. The play impulsion, on the other hand, may arise in a great variety of situations having little in common with one another, and may issue in an equally great variety of responses. McDougall has restricted the term 'instincts' to the relatively specific tendencies, classifying play with imitation and the like as 'general innate tendencies[1].' This seems a rather arbitrary and somewhat inconvenient restriction of 'instinct,' but it is at least an acknowledgment of the distinction. This difference in relative specificity certainly exists, but, as I have said, this character of specificity is the least important of the three as a basis for psychological classification.

2. *Appetition and Reaction.* The second character is in an entirely different category as far as human psychology is concerned. The basis of classification in this case has impressed itself in one form or another on psychologists of all periods and of the most diverse standpoints. Freud, for example, makes the conflict between the 'pleasure principle' and the 'reality principle' play a leading part in the development of the human personality. Myers[2] takes 'ipsi-affect' and 'actu-affect' as the fundamental and primitive forms of affect. In older times Bain, as representing the English associationist school, drew the distinction between the appetites and the instincts. Dugald Stewart, still earlier, representing the Scottish school, distinguished on almost the same basis between appetites on the one hand, and desires and affections on the other. We can trace the same distinction as far back as Plato and Aristotle, and it travelled down through the Middle Ages as the distinction between 'concupiscence' and 'irascibility,' which Descartes rejected in favour of his own basis of classification.

Unfortunately we do not find anything like the same agreement as to the precise nature and significance of the distinction drawn by these various writers. Probably, however, most would agree with Myers that the 'ipsi-affect' represents or expresses "the relation of the primitive self to organic movements" (I should prefer to say 'organic states' or 'conditions'), while "the actu-affects express its relation to locomotive activity." I believe that even this is sufficient to yield us a psychological basis of classification of the most fundamental significance, far exceeding in importance, both theoretically and practically, the self, herd, and sex classification.

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[1] *Social Psychology.*

[2] "The Evolution of Feeling," *Austral. Journ. of Psych. and Phil.* 1923, vol. I. no. 1.

But can we not go a little farther, and push the distinction explicitly right into the field of 'instinct-motivation'? I have elsewhere suggested that, though the main function of feeling is simply regulative, instinct feelings being in no wise different from feelings generally, there are, nevertheless, cases where feeling functions as motive, that is to say, where action originates in a feeling state and terminates in a feeling state. McDougall would apparently refuse to accept this view, and would maintain that action never originates in feeling, that conation is always primary[1]. But surely it is merely obscuring the psychological issues to refuse to recognize a fundamental difference in motivation between the man who goes a sea voyage merely for the pleasure it gives him—the pleasure of eating in an example cited by McDougall[2]—and the man who goes a sea voyage, with equal pleasure perhaps, in order to reach some proposed goal at the end of it. Of course it might be urged that this is so complex a case, involving a so highly developed mental organization, that primitive motives are obscured and overlaid, and in any case any conclusion we may draw need not hold at the instinctive level. All that may be true, but it is not difficult to find illustrations of the distinction at all levels. Is the motivation in hunger and nausea, in the case of the human being, not radically different from that in anger and fear, both as to origin and as to issue? In the *Republic*, Plato, in order to distinguish between the appetitive and spirited elements in mental life, makes Socrates relate the story of Leontius, the son of Aglaion. He saw some dead bodies on the ground—the executioner standing beside them. Feeling at one and the same time a desire to look at them and a loathing of the thought, he struggles with himself till at last, overpowered by the desire, he "opened his eyes wide with his fingers, and running up to the bodies exclaimed: 'There! you wretches! gaze your fill at the beautiful spectacle!'[3]"

It is not suggested that there is no element of the nature of the appetitive in a reactive tendency like anger. It may also be readily granted that, as we descend the animal scale, the distinction gradually disappears, and ultimately the two types of motivation become merged in one another. But surely no one can fail to recognize the two types in human nature and experience throughout its whole range. Personally I am not wedded to the terms 'appetitive' and 'reactive.' But I am convinced that no argument yet urged against the distinction is of sufficient weight to overbalance the fundamental facts, which appear to remain, even when we concede all that the opponents of the distinction claim.

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[1] See *Outline of Psychology*.

[2] *Op. cit.* p. 270.

[3] *Republic*, Book IV. 440.

In order to illustrate the practical or pragmatic value of the distinction let us refer once more to the Freudian psychology. The tracing of all love to sex love, and all sex love to sex appetite could not but arouse controversy and opposition. Most psychologists, and not least Freud himself, cannot help being aware of the difficulties which the position involves. Perhaps the most formidable of these difficulties is that which has led Freud to draw his distinction between 'zielgehemmte' and 'zielungehemmte' sex tendencies. Once more this distinction is in effect the same distinction—or virtually so—as we are drawing between 'reactive' and 'appetitive,' and as was implied in the classification in my *Instinct in Man* of 'sex' under 'appetitive' and 'courtship' under 'reactive' tendencies. With the deeper question whether Freud is right in deriving all from sex appetite we are not at present concerned.

3. *Relation to Emotion.* The scope of the distinction on the third basis is somewhat less than in the other two cases, but it also is of some importance, both 'academic' and pragmatic. McDougall has claimed that all the instinctive tendencies involve as one component the kind of experience we call emotion, and that this component is the most characteristic and permanent feature of the activity of the instinct. Whether we accept McDougall's teaching in its completeness or not, it is certain that some of the generally recognized instincts are related in a peculiarly close way to familiar and important emotions, so much so indeed that we can with considerable appropriateness designate the instinct by the name usually given to the emotion. Personally I am not prepared to regard an emotion as a definite psychological entity in the way McDougall apparently does. Nor am I disposed to limit the instinctive tendencies to such tendencies as show in their activity a characteristic emotional component. Rather I would take the view that the activity of an instinct involves instinct feeling or affect, and in proportion as the instinct response becomes protopathic in character—to use Rivers's terminology—the affect becomes emotional. All affect may become emotional. But

this does not tell the whole story. As we have seen, certain instincts are closely associated with characteristic protopathic response patterns—what McDougall has called the 'primary' emotions. Other instinctive tendencies are not so associated. Hence the need for the distinction between 'emotional' and 'simple' instinctive tendencies, or some similar distinction.

By the application of these three principles of classification we can divide the instincts on a psychological basis:

(1) into general and specific,
(2) under each head, into appetitive and reactive,
(3) under the last head into simple and emotional.

It ought to be noted that this classification embodies in whole or in part an old distinction between appetite and instinct, Freud's distinction between the pleasure principle and the reality principle, and between 'zielgehemmte' and 'zielungehemmte' sex instincts, Myers's distinction between 'ipsi-affect' and 'actu-affect,' McDougall's distinction between instincts and general innate tendencies, and Rivers's distinction between epicritic and protopathic instincts. All these distinctions are psychological, and within limits valid. It would seem an enormous gain if we could include them all in a relatively simple yet comprehensive schema. The classification is as simple and comprehensive as the three-fold biological classification. It has the additional advantage that it works, which cannot be said of the other, that it enables us to understand more fully in a psychological way, as distinct from a biological, the various instinctive tendencies of the human being. Thus it is not an 'academic' but a truly pragmatic classification. Finally, its basis is *a posteriori*, not *a priori*, the actual facts, not speculative hypotheses, and it enables us to group not merely the instinctive impulsions, or 'instinct-motivations,' but the instinct behaviour 'forms' as well.

THE CLASSIFICATION OF THE INSTINCTS (II)[1]

BY ERNEST JONES.

THE study of the instincts is in many respects the most fundamental in all psychology. It affords, to begin with, an approach to the central problem of philosophy, namely the relation of body to mind. Already through the study of the instincts and emotions psychology and physiology have come nearer together than ever seemed possible during the attempts made at cortical localization of the more intellectual processes. Then the same study brings us to the tremendous sociological problem of evaluating the relative importance of inherited and acquired tendencies. It is true that a recent school of thought, ably headed by Kuo and Kantor, seems to deny the existence of the former group by maintaining that all the reactions we describe under the term instinctive are really only acquired habits, but unless one is grossly misunderstanding these gentlemen they leave out of account some pretty broad facts of phylogenetic evolution as well as still more familiar ones of clinical observation. Finally, by investigating the most obvious resemblances between the mentality of man and that of the lower animals, this study offers a promising chance of winning for psychology recognition in its proper place in the hierarchy of sciences, namely as a branch of biology.

Of late years an extensive literature has grown up, particularly in America, dealing with the theory of instincts. It has become apparent that there are two broadly distinct schools of thought in this matter. There are those who, following Shand, concentrate on the motor aspects of instinctual[2] manifestations, and end in the extreme behaviourist position of ignoring all aspects but the visible ones. This attitude, curiously enough, leads to one of two opposite conclusions. Some behaviourists tend to regard most of these manifestations as being due to acquired habits, whereas others, such as Thorndike and Bernard, would postulate an exceedingly large number of innate determinants of reaction. The second school, headed by McDougall, are much more concerned with the purely psychological aspects of the subject, and seek—largely by means of introspection—for criteria to distinguish the various individual instincts. This results in one or other of the well-known conventional classifications. They are impressed with the obvious difficulty of correlating the observations of behaviourists with the introspectual descriptions, difficulties which have led many of the latter workers to deny the 'spontaneous' urges of the organism, and McDougall goes so far as to give up the belief in specific motor mechanisms corresponding with each instinct. Although I share Dr Drever's scepticism about McDougall's attractive but facile generalisation concerning the relation of specific instincts to specific emotions—the tendency of physiological research on emotions points strongly in the other direction—I feel that we owe a debt to McDougall for holding open another pathway from psychology to biology besides the route through physiology. As there is reason to think that the differences between the two schools of thought in question are, at least in great part, due to the temperamental attitudes of the individual workers, we may hope that a theory may yet be found which will comprehend both attitudes. A promising start in this direction would seem to have been made by Tolman's recently published conception of specific driving adjustments which set a particular goal for the random acts they tend to release.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

[2] I would plead for the acceptance of this adjective, meaning 'pertaining to instinct,' in distinction from the much more general 'instinctive.'

The greater part of the publications on this subject are taken up with discussions of principle, observation and analysis of actual phenomena playing a very subordinate part. In suggesting the theme of classification for this symposium I hoped that a little might be done to remedy this defect by testing the various theoretical schemes in their application to individual instinctual manifestations. It is therefore a matter of regret to me that the only contributor whose paper I have been able to see, that by Dr Drever, does not come to close quarters with this practical problem. Apart from his interesting general discussion he confines himself to expressing his disagreement with one particular classification, the ego, sex, and herd one[1].

Dr Drever finds that the classification into herd, sex, and ego instincts is 'altogether too alluring,' giving only an appearance of simplification, and I imagine he would pass a similar stricture on Freud's classification of sexual[1] and ego instincts. It is possible, however, that this simplicity may itself have a more profound meaning than appears on the surface. The point raises the whole question of the object of classification, as well as that of the number of the instincts. As Knight Dunlap has well remarked, if the object is that of convenient grouping for purely descriptive purposes it does not matter how large or how small is the list of instincts one makes, the only criterion being convenience for a particular purpose. Sociologists have notoriously taken full advantage of this licence, amid the shocked protests of the psychologists; two recent examples were the 'instinct for parliamentary government' and the 'instinct for salmon fishing.' If, on the other hand, there is a more ultimate goal beyond the classificatory attempt, the matter is on a different plane. If, for instance, the hope is to correlate one's classification with inherited biological and physiological tendencies one has to think of more objective criteria than temporary convenience. In this search for ultimate innate units, perhaps of a Mendelian order—of which as Barnard suggests, there are possibly many thousands—it may well be necessary first to traverse a stage when the numerous 'instincts' listed by various writers are reduced to a very few categories before the further task can be essayed of analysing the structure of these main groups into their elements. An analogy from the recent history of psychiatry will illustrate the point. Until twenty years ago the symptom-complexes met with clinically were variously listed according to the 'convenience' of the individual author, and it was difficult to establish any valid generalisations about them. Only after Kraepelin, followed by Bleuler and other workers, had reduced a large number of these particular syndromes to a few main groups, notably dementia praecox and the manic-depressive group, was it possible to begin analytic work on the pathology and aetiology of the various diseases. The analytic point of view, and the object of the classification thereby formed, may therefore be contrasted with the purely descriptive one.

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[1] Incidentally I think he is wrong in saying that this classification originates in psychopathology. It is rather a popular one. The concept of a herd instinct has been chiefly elaborated by sociologists, notably by Trotter, and so far as I know has been used by only two psycho-pathologists, Hart and MacCurdy. The other grouping into ego and sex instincts is a much older one; Freud, who uses it extensively, seems to have taken it from Schiller's well-known passage on 'hunger and love.'

If this is so, other possibilities arise. There is, for instance, much reason to think that the differences between the sexual and ego groups in Freud's classification may be of far more fundamental biological significance than those among the numerous elements into which these two groups will doubtless one day be dissected, and the differences may well be of a profound qualitative order. The main hint Dr Drever himself gives of a classification is after all into two groups only, the appetitive and reactive ones. The distinction is evidently a valid one, though how useful it may prove is another matter. As a criterion to be applied to the whole gamut of instincts it could be justified only if the difference between the two classes is both fundamental and important. Clinical observations make it doubtful whether the difference is fundamental. Thus many instinctual manifestations, for instance most sexual ones, are both appetitive and reactive, and Dr Drever himself admits that the distinction vanishes altogether as we descend the animal scale.

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[1] I would suggest that the terms 'sexuality' and 'sexual' be used always for general references to the instincts concerned, both the substantive and adjective 'sex' being reserved for their original reference to the fact that certain animals have divided their sexual life and organs into two sets, usually in two kinds of organism. The expressions 'sexual differences' and 'sex differences' would then have quite separate meanings.

After these introductory remarks I come to the main purpose of my contribution, which I conceive to be the statement of some of the respects in which psycho-analysis may be considered to have added anything to our knowledge of the present subject, particularly as regards classification.

(1) It should be said, to begin with, that the distinction Freud effects between sexual and ego instincts is primarily a psychological one, being based ultimately on the introspectual evidence of the specific sensations and perceptions accompanying various processes. That it should happen to coincide with the biological distinction most fundamental to all life, namely that between the impulses tending towards preservation of the self and of the species respectively, can surely not be regarded as a weakness in it. Freud has of late attempted the further correlation between the two groups and the tendencies towards a reinstatement of former quiescent conditions, ultimately of death, on the one hand, and those tending towards the furtherance of life in general on the other, but this is avowedly a speculative suggestion on which I do not wish to dwell here.

Psycho-analytic investigation of the unconscious mind, the region from which proceeds the greater part of our mental activities, leads one to attach vastly more importance than is generally done to (*a*) the instinctual and emotional elements as compared with the intellectual: in fact one can see no dynamic significance whatever in ideas except in so far as they function as the representative of some impulse or other; to (*b*) inborn tendencies rather than acquired habits; and to (*c*) appetitive impulses as compared with reactive ones. The weight of these conclusions is hard to appreciate without some knowledge of the importance of conscious activity in our mental life.

(2) The knowledge gained by these investigations strongly reinforces the prevailing scepticism about the lists of instincts still so often furnished

by psychologists and sociologists. Actual analytic examination of the manifestations attributed to these various instincts shows in many cases that an instinct supposed to be primary is not at all so, but can be resolved into further components. Thus the so-called instinct of self-abasement proves to be mainly compounded of masochism and fear, those of curiosity and possession to contain enormous sexual components, that of self-preservation or, as MacCurdy terms it, security to be closely bound up with narcissism, and so on. The often-vaunted 'instinct of power,' or which perhaps includes McDougall's instinct of self-assertion, seems to be an attribute of the whole personality rather than a separate instinct; certainly many separate impulses, such as indirect exhibitionism, come to expression under the guise of it. A study of its development shows conclusively that the parental instinct is not so distinct from the rest of the sexual life as McDougall and others would have us believe.

Further analytic study of the unconscious roots of the instincts commonly enumerated is bound to throw much light on this very question of which, if any, are primary, and which can be resolved into further elements. This is a further reason for preferring a tentative classification of a few large groups to the custom favoured hitherto of making arbitrary long lists.

(3) The contributions made by psycho-analysis mentioned so far have been largely sceptical or negative in character. Perhaps the most important single contribution of a positive kind is the demonstration that the normal sexual instinct is vastly more complicated in its structure and extensive in its ramifications than had previously been recognised. I must confine myself here to this mere statement, for an exposition of the respects in which this instinct has been shown to be broader and more important than is generally appreciated would occupy much more time than is allotted to me here. I am of course well aware that the statement in question has met with heated denial on the part of other psychologists, but one sometimes wonders at the temerity of those who can so nicely delimit the extent of the sexual instinct without finding it necessary to make any preliminary investigation of the subject. The conclusions enunciated by psycho-analysts are at least no dogmatic *a priori* pronouncements, but are based on years of extremely laborious and detailed research. The complaint is sometimes made that we tend to ignore our opponents: it is hard, however, to devise a more useful attitude until we are presented with evidence of serious work and not merely emotional vituperation.

(4) A contribution of special interest, particularly for the future, is

the series of studies published by Freud during the past few years which are devoted to the further analysis of the individual component parts of the sexual instinct. In them he traces the origin, development, and fate of each of these components, elucidating their complexity, and unravelling their inter-relationships. The work should serve as a paradigm for similar genetic investigations of other instincts. It was what I had in mind earlier when suggesting that the preliminary classification of large groups must be followed by highly detailed dissections of the constituent components of these.

Before concluding I should like to offer a few further points of view, without knowing how far other psycho-analysts might go in agreement with them. It would seem for many purposes useful to group all instinctual manifestations into two for which the tropistic terms of 'attraction' and 'repulsion' could be used, or in plainer English 'like' and 'dislike.' The former of these would comprise hunger and sexuality, with the two subordinate members of curiosity and possessiveness both of which I consider to be entirely derived from the two main ones. The second group, all ego instincts, would be made up of three: aversion, passing on to disgust; flight, with all the sub-varieties described particularly by Rivers; and aggressiveness, hostility, or pugnacity. I seriously doubt whether any manifestations of instinct exist which are not included in this scheme.

THE PROBLEM OF FORM IN PERCEPTION[1]

By WOLFGANG KÖHLER.

In dem Bestreben, ihr Gebiet auf möglichst einfache und klare Art zu begreifen, setzen junge Wissenschaften gern "additives Verhalten" in den von ihnen beobachteten Erscheinungen voraus, halten also möglichst an der Annahme fest, dass Zustände und Vorgänge im Grossen durch Ueber- und Nebeneinanderlagerung unabhängiger Zustände und Vorgänge im Kleinen zustandekommen. Ein gutes Beispiel hierfür ist die Entwicklung der Vererbungswissenschaft; vielleicht ein noch besseres bietet die Psychologie dar, wenn sie unsere Wahrnehmungen aus Einzelerregungen und Einzelempfindungen sowie gewissen Zusatz-Wirkungen der einzelnen untereinander ableitet, oder wenn sie den Vorstellungs- und Denkverlauf aus Superposition oder gegenseitiger Behinderung einer Mehrzahl selbständiger Verlaufstendenzen erklären möchte.

Da man jedoch auf diese Art zu einer höchst unbefriedigenden Vorstellung vom Menschen gelangt, so hat es an Einsprüchen nicht gefehlt: nicht nur müsse der ganze Mensch für eigentlich psychologisches Verständnis als eine Einheit gelten, auch relativ abgeschlossene Zustände und Vorgänge im einzelnen könnten ihrem Wesen nach nur als Ganze, nicht als Komplexe selbständiger Elemente begriffen werden.

Das ist ein *Problem*. Am schärfsten hat es sich bisher für das Gebiet der Wahrnehmung fassen lassen, und da hier Begriffsbildung und experimentelle Methodik an weitesten entwickelt sind, so hoffen wir, dass Klärung gegenüber der relativ leichten Aufgabe später auch zu einer Antwort auf die allgemeine Fragestellung führen wird.

I.

Mach, von Ehrenfels, von Kries und Becher sehen deutlich das Paradoxe in der anschaulichen Aehnlichkeit von räumlich und zeitlich ausgedehnten Gebilden, die zwar das gleiche Bauprinzip, aber wenige oder garkeine Empfindungsbestandteile gemeinsam haben, weil sie anders gefärbt, gelagert usw. sind. Ehrenfels gibt den Hinweis, dass Verteilung der Reize für solche Gebilde auf verschiedene Personen die charakteristischen Gebilde Eigenschaften zum Verschwinden bringt, dass dieselben Gebilde dagegen im wesentlichen sich gleich bleiben, wenn man sie—wie z. B. eine Melodie—transponiert. Von Kries und Becher betonen, dass solches Transponieren die Gedächtniswirkungen der Gebilde in hohem Masse unverändert lässt. Benussi weist darauf hin, dass sogar Gebilde verschiedener Sinnesgebiete bei gleichem Bauprinzip einander ähnlich und von ähnlicher Reproduktionswirkung sind.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923.

Dadurch dass bestimmte Gebilde angebbaren Bereiches und angebbarer Begrenzung diese phänomenale Verwandtschaft und diese übereinstimmenden Gedächtniswirkungen haben, wird man darauf aufmerksam, dass unsere Wahrnehmungsfelder und der Verlauf unseres Wahrnehmens eine deutliche Gruppierung in eben jene Gebilde als feste, aber von einander deutlich abgesetzte Einheiten im weniger innigen Gesamtzusammenhang der Felder und Verläufe überhaupt aufweisen. Selbst wer z. B. ein Gesichtsfeld aus sehr vielen lokalen Einzelempfindungen aufgebaut denkt, muss anerkennen, dass sich zumeist für die unmittelbare Beobachtung Gebiete grösseren Bereiches als Einheiten herausheben oder abgrenzen, ja dass diese natürliche Gliederung allein psychologische Wirklichkeit hat, während die Empfindungselemente der üblichen Theorie nur hypostasiert sind. G. E. Müller und Wertheimer haben eine Anzahl von Regeln angegeben, nach denen diese Gliederung geschieht: Räumliche und zeitliche Nähe, sowie qualitative Verwandtschaft von Reizen sind Faktoren, die im Sinn von Einheitsbildung wirken, vor allem aber findet man die natürliche Gliederung durch eine Tendenz zur Ausbildung "guter, prägnanter Gestalten" bestimmt (Wertheimer). Nicht alle Felder oder Verläufe zeigen eine klare Gliederung, regellose Reizvielheiten erzeugen vielmehr oft den Eindruck des "Chaotischen" und Hirnschädigungen bestimmter Art führen zu einer Aenderung in eben dieser Richtung. Auch gelingt es häufig, gegebene Gliederungen durch aktives Eingreifen (absichtliches Umgestalten) in andere überzuführen. Solche Erfahrungen aber lassen die Tatsache natürlicher fester Gliederung in tausenden von anderen Fällen nur noch bedeutender hervortreten, und an den Widerständen, die man beim aktiven Umgliedern einer gegeben Gestaltung zu überwinden hat, verspürt man erst recht, dass nicht alle Gliederungen gleichwertig, sondern im allgemeinen bestimmte Gestaltungen ausgezeichnet sind.

Gestalten haben, je nach dem Bauprinzip, bestimmte Eigenschaften oder Charaktere, die für jede Theorie additiver Tendenz unverständlich bleiben, wie "Dur" und "Moll" in der Musik, "Schlank" und "Plump" in der Optik, "Ruhig" und "Bewegt" nebst vielen andern auf den verschiedensten Gebieten. Rubin hat unsere Aufmerksamkeit auf einen fundamentalen Beschaffenheitsunterschied vor allem in optischen Feldern hingelenkt: Gestaltete Gebilde oder "Figuren" besitzen einen "festen"

Charakter (wie "Dinge"), durch welchen sie sich charakteristisch von der Leere, Unbestimmtheit, Wesenlosigkeit blossen "Grundes" unterscheiden. Die Verteilung dieser beiden Erscheinungsarten erweist sich an die jeweilige Gestaltung gebunden; genau das gleiche Feldgebiet, das eben "Figur" war, kann durch Veränderungen in der Umgebung sofort in "Grund" verwandelt werden, der Unterschied hängt also vom Bauprinzip im ganzen ab.

II.

Mehrere ältere Erklärungsversuche werden dem vorliegenden Tatbestand nicht gerecht.

(1) Die Hypothese *gleicher* (z. B. motorischer) *Begleiterscheinungen* als Ursache der (dann eigentlich nur scheinbaren) Aehnlichkeit von transponierten Gestalten ist an sich wenig wahrscheinlich. Sie schliesst ausserdem die Voraussetzung in sich, dass wir uns fortwährend aufs ärgste über unsere wirklichen Wahrnehmungen irren; und sie richtet sich auf eine spezielle Seite des Gestaltproblems, ohne die andern zu berücksichtigen oder gar zu klären.

(2) Wenn darauf verwiesen wird, dass transponierte Gestalten doch die *gleichen Relationen*, also gemeinsame Bestandteile enthalten, so ist zu entgegnen, dass an einer guten, festen Gestalt die vielen Relationen garnicht wahrgenommen werden, die sie enthalten soll, und einzelne Relationen im allgemeinen erst zu erfassen sind, wenn man die Gestalt zu Gunsten jener Beziehungen umbildet.—Richtig ist nur, dass beim Transponieren einer Gestalt ihre charakteristische Beschaffenheit erhalten zu bleiben pflegt, wenn das Gefüge der objektiven Relationen zwischen den Reizen das gleiche bleibt. Aber das ist eine Invarianzregel für Gestalten, nicht eine Erklärung.

(3) Es liegt nahe, das Auftreten fester einheitlicher Gebilde darauf zurückzuführen, dass ihre Teile oft in solcher Gruppierung zusammen aufgetreten sind und sich deshalb stark miteinander, ausserdem im ganzen mit andern Erscheinungen *assoziiert* haben. Denn viele Gestalten sind gewiss Formen von Dingen unserer Erfahrung. So würde indessen zunächst nur eine *Funktions*einheit und erst nach einem besonderen Ausbau des Assoziationsprinzips die *phänomenale* Geschlossenheit der Gestalten erklärt. Unerklärt bleiben ferner die charakteristischen vom Bauplan abhängigen Eigenschaften der Gestalten als solcher (Dur-, Moll-Charakter usf.). Endlich lässt sich durch geeignete Anwendung der oben erwähnten Regeln für phänomenale Einheitsbildung leicht erreichen, dass stärkste *empirische* Zusammengehörigkeit durch die natürlichen, *primären* Gliederungstendenzen überwunden wird.—Richtig ist, dass

früheres Wahrnehmen einer bestimmten Gestaltung (etwa unter günstigen Umständen) spätere Wahrnehmung gleicher Gestaltung (etwa unter ungünstigeren Umständen) erleichtert. Dieser Satz *erklärt* jedoch die Gestaltung nicht, sondern er spricht unter *Voraussetzung* der Gestalten von ihrer Nachwirkung.

(4) Ausgehend von der Beobachtung, dass sich Gestalten bei konstanter Reizlage durch aktive Einwirkung mannigfach verändern lassen, stellte Benussi die Hypothese auf, dass *alle* Gestaltung durch einen intellektuellen *Produktionsakt* zustande komme, der das selbständig vorhandene, aber ungeformte Empfindungsmaterial in beliebige Formen bringe; auch wo wir von einer solchen Tätigkeit nichts bemerken. Aber weshalb bemerkt man sie dann nicht, während in jenen Fällen aktiver Umformung die subjektive Bemühung garnicht zu verkennen ist? Ueberdies ist Gestaltung keineswegs beliebig, nicht alle Reizkonstellationen lassen überhaupt gute Gestaltung zu, und wo verschiedene Gliederungen gelingen, sind sie häufig sehr verschieden leicht zu erzielen, in verschiedenem Masse natürlich. Schliesslich wird durchaus nicht mit einem Material von festen Empfindungen eine Gestalt gebaut, vielmehr zeigt genauere Beobachtung, dass die lokale Beschaffenheit eines Gestaltgebietes häufig aufs stärkste von der Struktur abhängt, in der das Gebiet liegt. Dies wies Fuchs für Farben nach, eben hat es sich auch in Untersuchungen über die optischen Schwellen herausgestellt (Wertheimer, Gelb), und Benussi selbst erklärte ja schon die geometrisch-optischen Täuschungen daraus, dass die Beschaffenheit von Gestaltteilen Funktion der jeweiligen Gliederung sei. Nachdem sich endlich gezeigt hat, dass niedere Wirbeltiere in Wahldressuren gegenüber Paaren von Farben transponierend wählen, kann die Erklärung durch intellektuelle Tätigkeit wohl nicht mehr festgehalten werden.

Wenn das Gestaltproblem auch in der Tierpsychologie besteht, liegt der Gedanke nahe, dass passiv aufgenommene Gestalten auf physiologischen Erregungsvorgängen entsprechenden Charakters ebenso unmittelbar beruhen, wie die Empfindungen auf entsprechenden Einzelerregungen in der älteren Theorie. In der Vorstellung, physisches Geschehen könne die wesentlichen Eigenschaften der hier betrachteten Gebilde haben, wird man noch bestärkt durch die Tatsache, dass nach bekannten Befunden der experimentellen Morphologie das Verhalten von Keimen in ihrer frühen Entwicklung ausgeprägte Gestalteigenschaften aufweist.— Nun hat man daran gedacht, die Gestalteigenschaften von Wahrnehmungsfeldern aus der Funktion eines besonderen nervösen Apparates ad hoc zu erklären, wie ja ähnlich Hypothesen auch gegenüber den erwähnten entwicklungsgeschichtlichen Tatsachen ausgebildet wurden. Die im folgenden besprechene Möglichkeit hat mit einer solchen Maschinentheorie der Gestalten nichts zu tun, sondern leitet die allgemeinsten Gestalteigenschaften von Zuständen und Vorgängen aus der Natur zusammenhängender physischer Systeme überhaupt ab.

III.

Von Kries und Becher haben keine plausible physiologische Erklärung der auffallenden Eigenschaften von Gestalten gefunden. Unter der Voraussetzung, dass lokale Einzelreize lokale Einzelerregungen in isolierter Leitung zu bestimmten Endzellen schicken und hier die Empfindungsprozesse veranlassen, sind jene Eigenschaften auch wirklich nicht zu verstehen. Auch ohnedies ist die Ueberzeugung sehr verbreitet, es könne:

(1) physische Zustände und Vorgänge von dem Typus der wahrgenommenen Gestalten überhaupt nicht geben, und deshalb:

(2) auch nicht im Nervensystem Spezialfälle solchen Geschehens, sofern nicht bisher ganz unbekannte physiologische Prozesse entdeckt würden, die sich prinzipiell von denen der anorganischen Natur unterschieden.

Ich glaube zeigen zu können, unter welchen Umständen sehr mannigfache Gruppen von physischen Vorgängen die charakteristischen Eigenschaften wahrgenommener Gestalten zeigen, und dass die These 1 auf einem falschen Bild von der anorganischen Natur beruht. Dabei beschränke ich mich zunächst auf stationäre Zustände und Vorgänge, wie sie als physische Grundlagen *ruhender* phänomenaler Felder in Betracht kommen.

Man hat schon früher gesagt, dass "der Organismus sich mit einem Reiz ins Gleichgewicht setzt," d. h. dass eine Empfindungserregung im stationären Zustand das Gleichgewicht des betreffenden Nervenelementes gegenüber dem Einfluss des Reizes bedeutet. Aber ist das eine Angelegenheit des einzelnen Reizes und des einzelnen Nervenelementes? Und was muss eintreten, wenn mehrere lokale Reize zugleich einwirken? Hält man am additiven Prinzip streng fest, so ist die erste Frage zu bejahen, und im zweiten Falle entsteht folglich das Nebeneinander derjenigen Erregungen, die den einzelnen Reizen als zugehörige Gleichgewichte entsprechen, etwa noch kompliziert durch sekundäre Zusatzwirkungen, welche eine an sich selbständige Erregung auf die andere ausübt.

Ist das die einzige physikalisch zulässige Möglichkeit? Durchaus nicht. Denn wenn ein physikalisches System in sich zusammenhängt, so bestimmen nicht etwa lokale Einzelbedingungen ("Reize") lokale selbständige Gleichgewichtszustände, die nur noch etwa sekundäre Zusatzwirkungen auf einander ausübten. Da vielmehr das ganze Gebiet des Systems *in sich* so gut wie mit den äusseren Bedingungen im Gleichgewicht sein muss, so entstehen von vornherein garkeine lokalen Gleichgewichtszustände, sondern der innere Zustand im ganzen, prinzipiell *überall* unter dem Einfluss der *gesamten* Bedingungskonstellation, verschiebt sich solange, bis er als ganzer Gleichgewichtsgruppierung für das System und die gegebene Bedingungsmannigfaltigkeit geworden ist. Dann ist keine lokale Beschaffenheit selbständig für sich, wie sie ist, sondern nur, weil ihr in der gesamten Gleichgewichtsstruktur diese Beschaffenheit zukommt, und deshalb bildet der Zustand eines solchen Systems eine *Einheit*, für deren Bereich der angegebene funktionelle Zusammenhang besteht. Beispiele sind: die Formung von Flüssigkeitsgrenzen unter dem Einfluss von Oberflächenkräften, die Formung von elastischen Körpern unter dem Einfluss von Drucken und Spannungen, die Gleichgewichtsverteilung von elektrischen Ladungen auf Leitern, die stationäre elektrische Strömung in einem Leiternetz oder in kompakten Körpern, die stationäre hydrodynamische Strömung in begrenztem Raum.

Da es also ausgedehnte physische Einheiten gibt, in denen die lokale Beschaffenheit jeder Einzelstelle prinzipiell vom "Bauplan" des Ganzen abhängt, so findet sich eine erste Eigenschaft phänomenaler Felder in der Physik wieder. Jene Gleichgewichtsgruppierungen stimmen aber in noch weiteren Zügen mit der Gestaltung ruhender Wahrnehmungen überein.

Dass Zusammenhang in *einem* System vorausgesetzt ist, entspricht dem Hinweis von Ehrenfels, dass die charakteristischen Gestalteigenschaften verloren gehen, sobald die bedingenden Reize auf verschiedene Individuen verteilt werden.—Dieselbe physikalische Gleichgewichtsstruktur ist in sehr verschiedenem Material möglich (Strömungen z. B. in elektrolytischer und metallischer Leitung).—Verkleinerung oder Vergrösserung der Raumform, in der sich das Gleichgewicht ausbildet, lässt das Prinzip der Gleichgewichtsverteilung unverändert.

Die nächstliegende Annahme ist hiernach, dass auch These 2 falsch ist. Wenn nämlich der optische Sektor des Nervensystems wirklich ein zusammenhängendes System darstellt, so wird eine Mannigfaltigkeit von Reizen, die die Sinnesfläche treffen, nicht eine entsprechende Zahl von Einzelerregungen, sondern im allgemeinen schnell einen Gleichgewichtszustand im ganzen ergeben, welcher wie die physikalischen Beispiele die allgemeinen Gestalteigenschaften haben muss. Ihnen würden die Gestalteigenschaften des Gesichtsfeldes entsprechen. Dass sich im Totalzusammenhang eines Gesichtsfeldes kleinere Einheiten von besonders festem Bau abgrenzen, ist eine Erscheinung, die sich ebenfalls bei rein physikalischen Gleichgewichtsstrukturen wiederfindet.—Ueber die spezielle Beschaffenheit der Kräfte und Prozesse, die gerade im Nervensystem auf Gleichgewichtsverteilungen hinstreben, sind dagegen vorläufig nur vorsichtige Hypothesen möglich; es scheint, dass es sich wesentlich um osmotische, elektrochemische und Oberflächen-Kräfte handelt.

Wie man sieht, wird nach dieser Auffassung nicht mehr lokalen Erregungen durch Festlegung auf bestimmte Bahnen ihr Platz mechanisch zugewiesen, sondern in unmittelbarem Zusammenhang und nach den Gesamterfordernissen der jeweiligen Reizkonstellation bildet sich eine Gesamtgruppierung auf Grund autochthoner innerer Tendenz. Damit hängt das folgende zusammen: In der Physik haben Strukturen der angegebenen Art die Tendenz, sich in ganz bestimmten ausgezeichneten Richtungen zu verschieben, nämlich so regelmässig, streng, charakteristisch zu werden wie möglich, bisweilen unter Ueberwindung der Widerstände, die durch entgegenstehende Bedingungen gesetzt werden. Ganz unabhängig hiervon hat Wertheimer an phänomenalen Gestalten die gleiche Tendenz festgestellt. Dabei ist der Kreis als Endergebnis in der Physik wie im Gesichtsfeld besonders ausgezeichnet.

So scheint es, als ob der Satz, Physisches und Psychisches seien durchaus unvergleichbar (Wundt), in bezug auf Gestalteigenschaften durchaus nicht gilt; es taucht vielmehr die Möglichkeit auf, dass im Gegenteil jedes phänomenale Feld und der zugehörige physiologische Zustand in Struktureigenschaften einander maximal ähnlich sind. Das psychophysische Problem und mehrere psychologische Fragen würden dann ein stark verändertes Aussehen bekommen. Ebenso ergäbe sich ein neuer Weg, die Steuerung der Innervationen durch das jeweilige Wahrnehmungsfeld zu begreifen. Dies wie die Anwendung des Grundgedankens auf allgemein biologische Fragen kann ich hier nicht behandeln.

THE PERCEPTION OF MOVEMENT IN THE REGION OF THE BLIND SPOT

A REPORT OF EXPERIMENTS PERFORMED IN THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF GIESSEN[1]

BY K. KOFFKA.

Gestalt-Theorie gives a new impulse and in a way a new meaning to experimental Psychology. Wertheimer, who was the first to propound it, based it on his experiments on the visual perception of movement. Expose an object in the position *a* for a short time and after a brief interval (p) another object in the position *b*, the observer will see *one single* object moving from a position a' to the position b', provided you have chosen properly your experimental variables. As a rule a' and b' will not absolutely coincide with *a* and *b*. By a great number of different experiments Wertheimer proved that this impression of movement does not result from the reproductions of formerly seen real movements, that we must abandon all attempts to explain it by one of the customary psychological theories, *e.g.* completion or elaboration of sensations by the so-called higher functions. Perception of movement, so he concluded, must find the same explanation, whether this perception be provoked by real movement or by successive expositions of two unmoved objects. The perception of movement as a whole is a unitary reaction to the plurality of outer stimuli; physiologically, therefore, a unitary 'formed' (*gestalteter*) process must correspond to the phenomenal dynamic structure. Furthermore, as there exist phenomenally static besides dynamic structures, he also applied this inference to the static structures. If in the original experiment described above the exposed objects are two vertical straight lines in the positions *a* and *b*, and if you shorten the interval between the exposures of each of these, you reach a point where the observer, instead of seeing one dash moving from a' to b', sees two parallels unmoved. If, on the other hand, you increase the interval, the observer will see first line *a*, then line *b*. So, merely by the variation of p, we can produce three main stages, called by Wertheimer the successive, the optimal and the simultaneous stage. In this order these are stages of increasing connectedness or formedness (*Gestaltung*).

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July 1923. A full account will be published in the *Psycholog. Forschung* by Frl. A. Stern.

In an investigation carried out in the Psychological Laboratory of Giessen, A. Korte examined the dependency of the impression of movement, or more generally of the degree of 'formedness,' upon the different variables, such as the spatial distance of the stimuli, their temporal relations, *i.e.* the duration of exposure and the interval, and their intensity. He discovered a number of very simple laws which in an investigation carried out by Dr Armak and myself were found to hold for the perception of real movement also; at the same time we succeeded in finding a complementary law and in bringing the Korte laws to quantitative terms.

These and some other experiments seemed to confirm Wertheimer's theory in a manner which excluded any other explanation. In spite of this the theory has not been generally accepted. In the face of our findings other investigators have urged the fact that our phenomena are by no means dependent on the stimuli alone, but are largely influenced by the observer's (*O*'s) subjective attitude and attention. This proved, so it was inferred, their intrinsic mental nature.

However, though these criticisms are quite true, they do not necessarily lead to the conclusion drawn, but are absolutely compatible with Wertheimer's theory. All phenomena, just as their physiological correlates, depend upon the totality of existing conditions; and attitude, attention, etc., form a part of the inner, the subjective conditions. Moreover, can we adduce empiric evidence that this interpretation of the facts is the true one? This leads us to a very general problem. If, before the application of the stimuli, the *O* has a definite setting of his attention, then his entire field of vision is influenced by this very setting. Let the objective field be entirely homogeneous with the exception of a fixation point and concentrate your attention on this point, then your visual field will be distinctly non-homogeneous, possessing a very narrow area with weight and an extensive diffuse ground. We may, then, put the following question: What is the effect exercised by the quality of the field upon the form-process?

The independent variable of this problem is open to definite experimental treatment, while attention can at best be fixed by *a*, the specific instruction given to the *O*. We can objectively form the field and so by objective means also determine the attentional setting.

From this point of view, the influence of the field upon the form process, I will here communicate some experiments on visual movement recently carried out in my laboratory by Frl. A. Stern. Even if the objective field be absolutely homogeneous, the subjective one is not, owing to the functional differentiation between centre and periphery of the retina. Besides this non-homogeneity which may be represented as a continuous function of the distance of any retinal point from the *fovea centralis*, the monocular field of vision comprises a region in which the function is strongly discontinuous, *i.e.* the blind spot. It has been Frl. Stern's task to investigate movement-phenomena in the region of the blind spot; she has been able to obtain results of importance as well for the theory of movement as for our knowledge of the blind spot. She worked chiefly with 'apparent,' kinematographic movement, *i.e.* she exposed successively two dots of light, one vertically above the other, both in a definite relation to the blind spot. Either (*a*) the points were situated so as to enclose the blind spot, lying on or near its lower and upper margin, or (*b*) one of them fell within the spot, or (*c*) both were depicted on the blind area.

That in case (*a*), both points outside the spot, optimal impression of movement could be obtained, was the result of the very first experiments. By observing the same constellation with the other eye in which the two points were not depicted on the blind spot, it was found that the blind area is a region particularly fitted for the perception of movement. The dots were exposed by means of a rotating strip in which slits were cut, the slits allowing the light to pass from a lamp to the pin-holes in the front screen which served as light-dots. We could vary the speed of revolution of the strip and determine the slowest and the fastest speed, and consequently the zone of speeds which produced the optimal movement. Comparing the optimal zones of the 'blind' and the 'normal' eye, the former was always found to be greater and in most cases considerably so. Moreover, this extension of the zone was not symmetrical but involved chiefly the slower speeds. Slower speed means longer interval between the exposures, and this means, as we have seen before, a change of conditions towards the successive stage, in other words towards a looser degree of 'formedness.' As, therefore, we see optimal movement over the blind spot region under conditions that produce on the corresponding eye the impression of bare succession, we can conclude that the field-conditions for close 'formedness' are specially favourable in the blind region.

Also phenomenally the movement through the blind area has a specific aspect. It is not a fugitive passage but has a strongly dynamic and energetic character of rare impressiveness: the point seems to move with great energy against strong resistance. Most striking, however, is the following fact. If anywhere in the field of vision outside the blind region two dots, one above the other, are successively exposed so as to produce the impression of movement, then this movement always takes place in a straight line, from below upwards or from above downwards. Only in the case when a blind region lies between the dots, the result is different: then the movement is always curved. The direction of this curve is determined by two factors, which have different respective weight with different individuals: (1) the distance from the margin of the blind spot; (2) the direction towards the fixation point. Consequently, if the two dots lie between the inner margin of the spot and its vertical median, the curve is always convex towards the fixation point; if they lie on the outer side of the median it is for some *O*'s always concave towards the fixation point while with others it is only so in rare cases, remaining generally convex as in the first case. The same constellation observed with the other eye always produces movement over a straight line; binocularly observed the movement is slightly curved but decidedly less so than in the first case.

If we increase the distance between the two points, removing them more and more from the margin of the blind spot, the curve becomes more and more stretched, approaching a straight line running right through the blind region. In all cases the movement terminates in a place *b'* distinctly different from the objective position of the second dot *b*.

Our inference is that this non-homogeneity of the visual field influences the form of the movement. It is important to note, that the same effect can be produced by introducing non-homogeneities into the objective field, to be observed with the fovea. Simplifying Linke's experiment, in which a point is forced to move round the simultaneously exposed arch, I could prove that this also can be explained only by the direct influence of the field upon the movement process. So, too, it seems impossible to explain the curves in the blind spot region 'psychologically.'

Here the following objection might be raised: since within the blind spot one can see nothing, so any movement taking place in its neighbourhood must take its course around the blind area. This, however, is untenable in the face of the result previously stated, that the curves approached the shape of straight lines and did undoubtedly pass over the blind region, when the distance between the two points was increased. I can, however, adduce more decisive evidence still. We introduced into the visual field faint and diffuse figures surrounding the blind spot and the two dots. Choosing circles or ellipses we found that the movement ran along or quite near to their periphery. When, however, squares or oblongs were exposed, then the track of the movement was absolutely straight, running right across the blind spot. We have, therefore, found that an objective change in the field combines with the subjective field conditions, and that in consequence of this combination movement may very clearly be seen through the centre of the blind area. It is noticeable that the influence of these figures is more pronounced on the 'blind' than on the 'normal' side, which proves anew that the blind region is particularly subject to the influence of 'forming' factors.

What will happen when one of the dots or both of them is exposed within the blind spot itself (cases *b* and *c*)?

Shift one of the points gradually into the blind region up or down to its very centre, leaving the other near the upper or lower margin, and choose your exposure times properly, then the *O* will get the optimal movement impression with the typical curve. If the marginal point is the first exposed, then you will see a distinct dot falling (or rising, as the case may be) into the blind spot and getting at the same time more and more diffuse, like a comet's tail. If, on the other hand, the point inside the spot is the first, then you see something very diffuse emerging from the spot and crystallising into a point. The marginal point exposed alone does not move (performing only slight movements of expansion and contraction), the point within the spot exposed alone is invisible, and both successively exposed produce the movement-impression just described!

These experiments proved also that the blind-spot is not a homogeneous region in itself. Only with comparatively strong intensities of illumination could the one point be shifted to its very centre and still produce the movement process. With weaker intensities it had to remain nearer the margin, otherwise its exposure in the sequel *ab* or *ba* proved ineffective. In fact the effective distance from the margin is a direct function of the intensity.

Even when (case *c*) both points are situated within the blind spot, their successive exposure produces an impression of movement. The *O* sees a big and very diffuse spot moving in a marked curve.

Our general conclusions are the following:

1. The blind spot is not absolutely blind, but merely highly insensitive to light-stimulation, the sensitivity increasing slowly from the centre to the margin.

2. This sensitivity is ever so much greater for double exposures resulting in movement-processes than for single or simultaneous double exposures.

3. It is impossible to explain these movement-impressions by 'higher psychological functions' built on the simple sensations, as in the cases described these supposed simple sensations cannot be produced.

I may add that these experiments were roughly repeated with 'real movement,' a light-point being rapidly drawn across the blind spot, and yielded the same results; the apparent movement took place in just the same curves.

'INTELLIGENCE' IN MOTOR LEARNING

BY M. GOPALASWAMI[1].

SYNOPSIS.

I. Purpose of research (p. 274).
II. First experiment: mirror drawing (pp. 274–282).
  1. *Apparatus and procedure* (pp. 274–278).
  2. *Results* (pp. 278–282).
III. Second experiment: mirror groove tracing (pp. 282–285).
  1. *Apparatus and procedure* (pp. 282–284).
  2. *Results* (pp. 284–285).
IV. Introspection (pp. 285–287).
V. Theories (pp. 287–290).
  1. *'Mechanical' theories* (pp. 287–288).
  2. *'Rational' theories* (pp. 288–289).
  3. *Corollaries, theoretical and practical* (pp. 289–290).

I. PURPOSE OF RESEARCH.

THE immediate aim of the present investigation is to verify experimentally such important theories as those advanced in recent years by Watson[2], Perrin[3], Snoddy[4] and others, as to the general nature of motor learning. Incidentally, an analysis will be gained of the procedure which is commonly called learning by 'trial and error,' and to which many authorities attach an extremely high importance.

II. FIRST EXPERIMENT: MIRROR DRAWING.

1. *Apparatus and procedure.*

This experiment was conducted with the help of an apparatus in some respects very similar in construction to the one used by Snoddy. The learning consisted in acquiring the movement responses necessary for tracing over a star pattern while seeing it through the medium of a mirror.

---

[1] This research, although conducted in the Psychological Laboratory of University College, London (under my supervision), was finally written up by the author in India. It was then sent to me for publication, with authority to make any alterations I thought necessary. These had to consist in reducing the paper to about a quarter of its size, in order to become suitable for the present *Journal*. C. SPEARMAN.

[2] *Psychology from the Behavioristic Standpoint.*

[3] "Conscious analysis *v.* Habit hierarchies." *J. Comp. Psy.* 1921.

[4] "Trial and Error Learning." *Psych. Mon.* XXVIII. 1920.

A star pattern is drawn on a piece of tracing cloth and held tightly on a fibre frame. The pattern is marked by 24 spots at regular intervals as shown in Fig. 1, below. These spots serve as intermediate points through which the subject's stylus must pass. The frame is mounted over a plate, and below the plate is an electric hammer in circuit with a metronome which makes an electric contact once every second. At each contact, the hammer hits the plate. Between the plate and the star pattern, a sheet of paper and one of carbon are inserted. By this means a record is taken of all the movements of the stylus over the pattern; furthermore, the various positions at which the stylus rests at the end of every second may be seen in the carbon tracing (as thick dots).

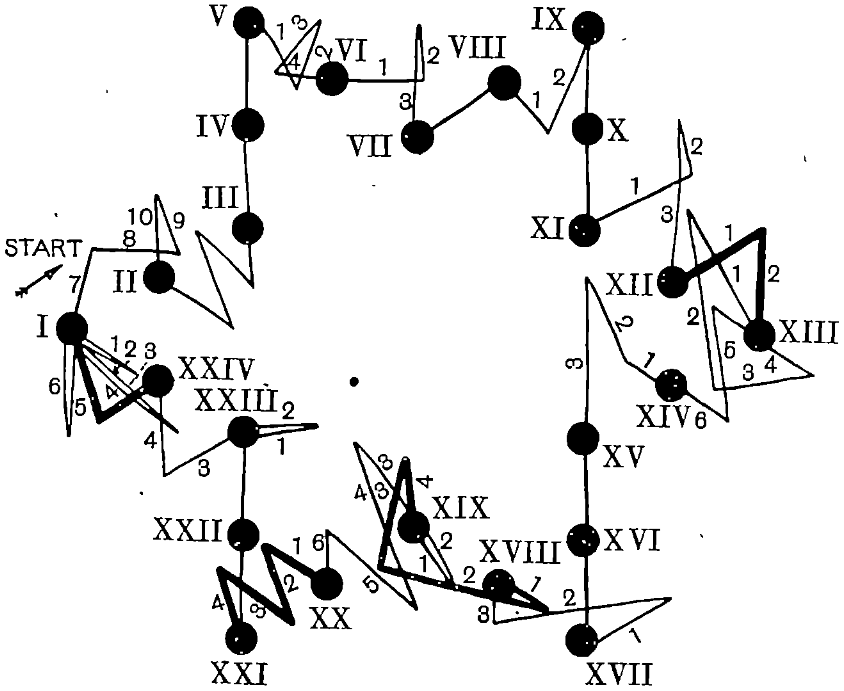

Fig. 1. An exact copy of the carbon tracing of all the movements of one subject in his first attempt at learning the star pattern. (Mirror drawing.)

NOTE. Where the movements belonging to different points on the star pattern are likely to be confusing they are shown in different thicknesses. The individual movements are numbered in the order of their occurrence.

The total number of movements made are counted from the tracing and taken as the measure of learning effort.

There is a check supplied by a simultaneous record kept on a kymograph. Here, two pens are placed over the paper ribbon running in the kymograph; the top one is in circuit with the metronome and marks the seconds; the bottom one is in circuit with an electric bell and a tap key. The experimenter can at will simultaneously ring the bell and mark on the kymograph, by pressing down the tap key. As he is closely watching the progress of the tracing, he can press the key at the beginning and also when the subject touches the various points in the star pattern. By a comparison of this time record with the number of actual movements made in the carbon tracing, any loss of rhythm or other irregularity can be detected.

The instructions—thoroughly explained and demonstrated to the subjects—were as follows:

(*a*) Only straight movements are allowed, never make a curved movement.

(*b*) Always keep in rhythm. Do not go too fast or too slow. Stop after each stroke. Move in jerks. Do not stop too long to think. You must make a movement for every stroke of the hammer.

(*c*) If you go the wrong way, correct from where you are. Do not go back to the starting point. *Also do not correct at once*, but wait for the next stroke of the hammer and then make the correcting movement.

(*d*) Always aim at getting to the centre of the spot. If you shoot over a spot, you must come back to it.

(*e*) When you get to a spot correctly, I shall ring the bell. Until then you must go on making rhythmic movements to get there.

(*f*) Always hold the stylus in a vertical position and freely slide your whole arm over the rest.

It was found necessary to insist on the observance of these detailed instructions by all subjects, as uniformity of method was essential to get any significant results. In the earliest experiments (not reported here) the latitude allowed to the subjects to attack the problem in their own individual manner introduced so many disturbing variants, that the results were wholly clouded.

For instance, the way in which the stylus was held, or the way the whole arm or only the wrist was moved, seemed to affect the results noticeably. Again, the subjects who withdrew to the starting point, instead of correcting the wrong movement from where they were, showed themselves to be at a great disadvantage. In giving these detailed instructions and making the subject fairly conversant with the procedure, a preliminary training on the first day was unavoidable. This, however, was very brief, lasting only three to four minutes. It was given on a square pattern, with the mirror at a slight angle, so that the square pattern was slightly deflected in the mirror, as indicated in the following Fig. 2.

We have not observed any interference exercised by this training upon the regular test.

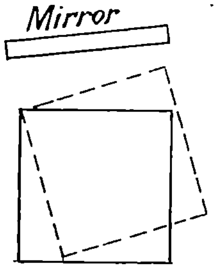

Fig. 2. The continuous line indicates the real square; the dotted line, the apparent square.

The subjects were urged to consider the performance as a sort of game; and it was explained how important it was that they should keep fairly to the rules. In order to make them put forth their best efforts, their spirit of rivalry was excited. After each day's test, their scores were put on a graph paper and they could see at once how they stood in relation to one another.

It will be noticed that our restriction of the subjects to rhythmic and straight movements only is a departure from the usual way of mirror drawing. This change of procedure was found to confer the following advantages. First, it reduces time and error measurements to one category, namely, that of the number of movements taken to traverse the pattern. In the more usual procedure, only time measures are taken and the errors are neglected, as the latter are very hard to measure. In this way, the record shows the anomaly of a seeming set-back after the first few runs; really, such a loss in speed is more than compensated by a gain in accuracy. Moreover, even when separate measures are somehow obtained for speed and accuracy, there appears to be no way of combining them except in an arbitrary manner.

The second advantage of such standardisation of method for all subjects serves to minimise the disturbing effects which otherwise the individual differences in temperament, fatiguability, and emotionality have on the work. There are subjects who have a natural tendency to be slow and deliberate in their movements, and others who are quick and impulsive. Such idiosyncrasies tend to obscure the significance of the results, if the interval between the successive reactions is not kept uniform for all subjects. Further, the rhythmic movement had the effect of delaying the onset of fatigue.

There is yet another advantage in this procedure of allowing only straight and rhythmic movements. It slows down the mental processes, so that we can now examine each phase of these at our leisure, much as a Motion Study expert would slow down the film of a complicated motor performance in order to analyse it. We are thus enabled to study the types of response, as also their correlation with the various stages of practice.

Complete learning was, for the purposes of the present research, constituted as follows. The subjects were given three runs a day on the star pattern. Between the runs were allowed intervals of not more than 30 seconds, *i.e.* the time taken for putting a fresh paper under the pattern frame. The practice was continued every weekday until the subject had attained sufficient mastery to go round the whole pattern of 24 points twice consecutively in at most 28 strokes each time (learning to go round without any mistakes at all, that is, in 24 strokes, was found to take too long a time).

2. *Results.*

We will now turn to the psychological analysis obtained by the procedure just described. For each movement of the subject as indicated by the carbon trace, the attempt was made to infer the nature of the mental antecedents that would naturally have prompted just such a movement and no other.

For this purpose, use had been made of a recent work, in which all cognitive operations are reduced to six ultimate elementary processes[1]. Of these, only three play prominent parts in the present case; they have been formulated as follows:

> *A.* The presenting to mind of any two or more characters evokes a knowing of relation between them.
>
> *B.* The presenting of any character together with a relation evokes a knowing of the correlative character.
>
> *C.* The presenting of any character reproduces some other character previously associated with it.

We will now apply this analysis to the actual tracing of one of the subjects. In Fig. 3 he starts from *I* and ought to go to *II*. Instead, he goes first to *X*, and then back to *I*. Evidently, the movement *I–X* accords with the normal coordination between hand and eye. Such coordination has been so thoroughly practiced in every direction all through life, that now it may be considered to have become almost purely a matter of habit. It therefore can be regarded as an instance of process *C*. Movement 2 (*X–I*) is a withdrawal back to the starting-point. Such withdrawals are among the commonest forms of reaction, after a wrong one. It can be explained by supposing that the previous wrong movement was perceived to be such (process *A*), and that then the relation of withdrawal is reproduced by virtue of being a habitual reaction in such circumstances (process *C*), and that

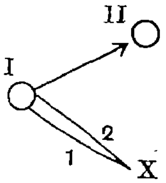

Fig. 3.

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[1] *The Nature of 'Intelligence,' and the Principles of Cognition*, by Spearman, London, 1923. Macmillan & Co.

finally this relation together with the perceived present position at X jointly evoke the actual movement of withdrawal (process B).

The next two movements, as depicted in Fig. 4, are similar, except for their greater length. This can be attributed to an exaggeration of effort.

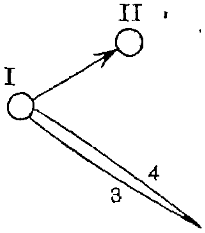

Fig. 4.

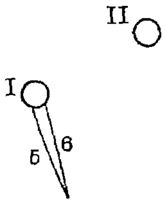

Fig. 5.

The next movement (No. 5), as depicted in Fig. 5, is of a very different kind. It admits of explanation as follows. A memory is present of the error already committed twice, which had been perceived (process A) to consist in the desired movement laying something less than a right angle anti-clockwise of the movement actually executed. This anti-clockwise relation together with the present perceived position of the tracer evoke conjointly, as the correlate, such an impulse as would normally produce the appropriate anti-clockwise correction (process B). But owing to the mirror, the change of movement actually effected has the reverse effect, namely, a shift clockwise. It should be observed that this constituent process B is far more novel and complex than the B involved before in the mere withdrawals. The next movement, No. 6, is obviously nothing but a withdrawal again.

As regards movement 7 (Fig. 6) the chief process involved appears to be some awareness at last that the mirror engenders a vertical inversion (process A). Accordingly, the subject, since he sees that his stylus should move slanting upwards in the mirror-space, makes actually the movement that would in ordinary space take it slanting downwards. This constitutes an instance of process B. But it is executed inaccurately, the allowance for the slant being insufficient. Movement 8, in the same figure, is the utilisation of a perceived part-relation, viz. that the objective lies to the right (process B). Movement 9 is simply a habit response once more. Movement 10 depends, like movement 7, on an awareness of the vertical inversion; but in the present case, such a simple awareness is all that is required.

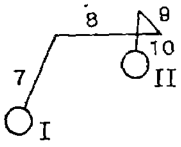

Fig. 6.

So much for the subject's first 10 movements. Now, there were 69 further movements made in order to complete the whole star. Of these, no less than 64 are explicable in just the same way (and just as naturally) as the first 10.

Let us, then, turn to the five movements that exhibit more or less difference of type. The first of these was movement 3 in travelling from hole XIII to XIV (Fig. 7). It can readily be explained on the supposition that the relation of inversion, which properly belongs to the vertical direction, being here applied to the horizontal.

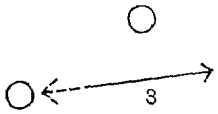

Fig. 7. The actual movement is drawn continuous; the correct one, dotted.

The second exceptional movement was No. 1 in passing from hole XVII to hole XVIII. It evidently consists in applying the inverting relation to *both* the vertical and the horizontal directions, instead of (as was proper) to the former alone.

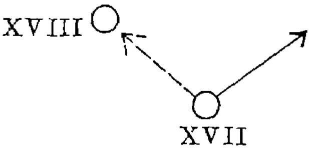

Fig. 8.

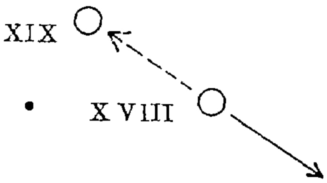

Fig. 9.

The third exceptional movement was No. 1 between holes XVIII and XIX. It consisted in applying the inverting relation to the horizontal direction and not to the vertical one, instead of *vice versa*.

The fourth and fifth exceptional movements were Nos. 1 and 2, from hole XXIII to XXIV. It consisted in a deliberate attempt to escape the difficult diagonal movement by making instead two steps, each of them very easy; the first is altogether normal; the second involves only the now well-known simple inversion vertically.

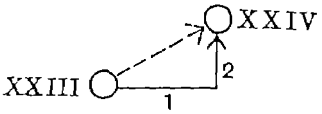

Fig. 10.

So far we have considered only one subject. On extending our examination to all ten, in each case including the learning of the whole pattern, only two additional types of response were discoverable. For one of these—very infrequent—no evident explanation on the preceding lines was forthcoming; it looked quite wild. But here introspection came to our aid. Such wild movements seemed to derive from the occasionally occurring impulse of despair to move *anywhere*. Hereupon the first movement presented to mind (reproductively by process *C*) was uncritically carried into execution. It used to be accompanied by lively emotions. The second additional type of reaction was a failure to move at all; this failure seems to have been due to inhibition and, like the preceding type, to have had an emotional character.

Altogether, the wrong reactions, some 500 in number, all fall into six similar types. And these can be arranged in two levels, higher and lower respectively, according as it does or does not involve any novel process:

I. *Lower level*

(1) Habit
(2) 'Wild'
(3) Inhibition
(4) Withdrawal

II. *Higher level*

(5) Correlate-finding, based on misapplying the relation of inversion
(6) Correlate-finding, based on misapplying the angular correction

It seemed desirable to ascertain how these two levels of response were affected by occurring near to the beginning or to the end of the whole star. With this aim, we have taken the diagonal sections of seven subjects in their first two runs on the first day (the vertical and horizontal sections were omitted, as being too easy for our purposes). Summing the number of mistakes at each level for the eight diagonal sections in succession, there ensues the following table.

No. of section	1	2	3	4	5	6	7	8
Direction of proper movement	↗	↘	↗	↘	↙	↖	↙	↖
Lower level response	89	37	36	30	27	32	54	38
Higher level response ...	24	10	12	14	26	16	17	6

Turning these values into percentages, expressing them in smoothed curves, and indicating that the first half of the performance was characterised by greatest improvement through practice, whilst the second half showed signs of fatigue, we get the following rather remarkable graph (Fig. 11).

There is thus evidence that the reactions of lower level tend to occur more readily, either at the beginning while the subject is still completely under the influence of life-long habit, or towards the end when he is becoming influenced by a newly-formed habit besides probably getting fatigued.

Of interest, further, is that success of the different children correlated highly with estimates obtained as to their intelligence, the coefficient being no less than ·81[1]. This value is so large as to have significance even with the very small number of subjects. Of course, a larger number of these would have been better. But it must be remembered that, owing to the elaborateness of the procedure, many hours had to be spent on each subject, so that to get through any large number of them was, humanly speaking, impossible.

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[1] Calculated by Spearman's "Footrule" and the Conversion tables given in Whipple's *Manual and Mental Tests*, p. 44.

On examining the results in detail, it appeared further, that the responses of the lower level persisted longer in the subjects who were estimated as low in intelligence. Some experiments also seemed to indicate that when the interval between the successive reactions was made shorter, the responses of the lower level became comparatively more frequent.

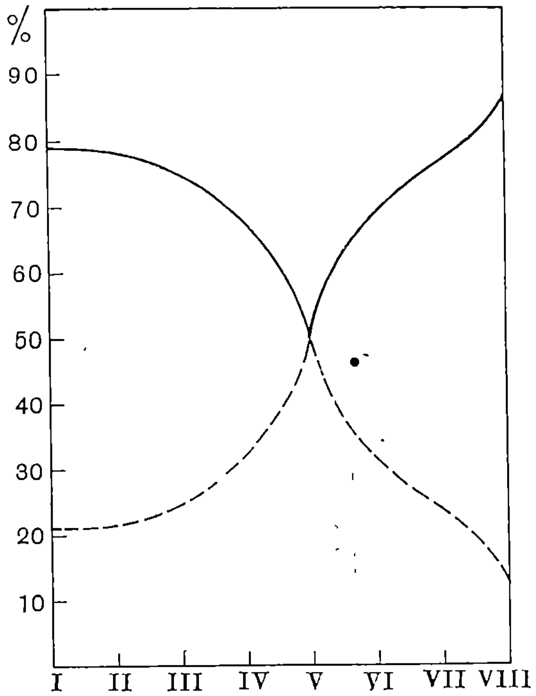

Fig. 11.

Lower level ————

Higher level - - - - - - - -

III. Second Experiment: Mirror Groove Tracing.

1. *Apparatus and procedure.*

The purpose of this experiment was to observe the effect of reducing to a minimum all the opportunities of the subject to judge the nature of his erroneous response. Under such conditions the learning—though otherwise comparable with that of the Mirror Drawing—was expected to afford less scope for the cognitive processes of higher level (see p. 281).

On a square piece of wood, visible to the subject in a mirror only, a narrow groove was cut of the same shape and size as the first half of the star pattern used in the previous experiment. Leading from this true path, there were some blind alleys placed at various critical points and turnings. The true path (but not the blind alleys) was marked prominently on both sides by a thick black line. The blind alleys were shaped in three types, based on the most common errors that all subjects tend to make when free movement is possible as in the experiment of Mirror Drawing.

(*a*) *Habit error.*

The blind alleys of this type were meant to catch those subjects who simply reacted on the basis of the normal and habitual coordination between hand and eye. Thus, if *A–B* is the true path, it would appear in the mirror to lie in the direction of *A–C*.

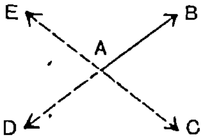

Fig. 12.

All strayings into the blind alleys of this type are here described as belonging to the 'habit' error group (see p. 281). This type of behaviour is the least 'intelligent,' in that it makes no allowance at all for the mirror space.

(*b*) *Error inversion.*

There exists, as we have seen, a strong tendency, when a movement fails, to move in the opposite direction. Early in the experiment, the original and the opposite movements used to occur in immediate succession, the second or opposite movement appearing to be a response of long-established and of very general character; it was called by us that of 'withdrawal' (see p. 278). But the success attending such a reversal in the case of the vertical lines induced the subjects to attempt an inversion of movement in other cases also; this tendency was perhaps strengthened by some notion that the mirror space was in some way the reverse of ordinary space. To catch reactions following this tendency we have placed the blind alleys as shown in Fig. 12 by the direction *A–E*.

(*c*) *Error of angular correction.*

The third type of blind alley is placed at right angles to the apparent path but opposite to the true path, as shown in Fig. 12 by *A–D*. This is the error that would occur if the subject noticed his previous deviation from the true path and endeavoured to correct for the deviation in the ordinary manner, that is to say, without taking also into account how the mirror space would *invert such a correction.* This is, in a sense, the most 'intelligent' of the erroneous responses, since the chief process involved has the greatest novelty and complexity.

There were 26 of these blind alleys in all, ten being of the habit type, and eight each of the other two types. They all had brass stops in them so very near to the entrance, that on entering them the subject could not see in which direction he had deviated from the true path; he only found himself unable to move further. These stops incidentally served as electric contacts. The three types of blind alleys were, by means of the stops, wired up in three separate circuits, so that every time the stylus touched a stop the corresponding circuit was closed and the type of error was registered on the kymograph. By the action of the same circuit, an electric bell began ringing, and thus at once brought to the notice of the subject the fact that he had gone astray. There was also on the kymograph a fourth pen connected to a metronome, so that the passage of every second was marked on the record.

In the present experiment, the learning was taken as completed when the subject made two consecutive runs without a single error. The subjects fell into two groups, as follows:

(*a*) One group ('massed' method) was allowed four runs on the first day and ten runs on each succeeding day, until completion of the learning.

(*b*) The other group ('distributed' method) was allowed two runs on the first day and five runs on subsequent days.

2. *Results.*

(*a*) *The following table shows the degree of success of the different subjects by the two methods.*

Massed method				Distributed method			
Subject	Runs	Time	Error	Subject	Runs	Time	Error
A	17	691	261	K	13	306	47
B	25	800	—	L	20	624	73
C	27	1098	394	M	25	723	120
D	21	1143	526	N	34	982	247
E	20	1145	192	O	14	1029	178
F	27	1573	553	P	19	1047	465
G	18	1834	430	Q	11	1280	192
H	22	1911	123	R	22	1736	546
I	13	2052	464	S	26	2039	325
J	49	2590	653	T	24	2050	727
Ar. mean —	24	1484	411	—	21	1182	292
Ar. mean deviation —		508	137	—	—	475	180

The preceding table would appear to indicate an advantage for the distributed method on all counts. In view of the big scatter, however, it would be desirable to obtain a larger number of cases.

(*b*) *Correlation with estimated intelligence.*

The next point to be mentioned is that on this occasion, in striking contrast to the previous experiment, success shows no correlation with the teacher's estimate of the children's intelligence. The coefficient—if one may venture to mention it for such small numbers of subjects—was only ·05 for each of the groups (as opposed to ·81 in the first experiment, see p. 281). This result is in good harmony with the view that the present procedure, by reducing to a minimum the subject's opportunity to judge the nature of his mistakes, thereby brought down his reactions to a lower average level (see p. 282).

(*c*) *Analysis of incorrect movements.*

Further light on this matter can be obtained by analysing the incorrect movements into the three types, as described on pp. 278–282. This has been done below for the first four runs.

Type of error	Angular correction	Inversion	Habit
Distributed method	207	261	1157
Massed method	208	430	2729

From this it is evident that the distributed method produces a much larger proportion of responses of the higher levels; whereas the massed method causes a large increase in the responses of lower level, especially those of mere 'habit.' One might infer that the poor success of the massed method is chiefly due to the fatigue and perseveration induced by it[1]. This view accords well, too, with the conclusions of other experimenters that the distributed method leads to greater *variety* of response. Thus Book, as a result of his investigation of typewriting, concludes that concentrated practices lead to ruts and limit the spontaneity of movement. Lashly, in a similar way, observes on the basis of his Maze Experiments that a greater diversity of activity occurs where the successive trials are separated by a considerable interval of time than when they follow each other immediately[2].

IV. INTROSPECTION.

So far, our evidence concerning the mental processes involved have been almost wholly of inferential nature; we have noted the subject's actual movements, and have surmised their mental antecedents. But this procedure—in human psychology at any rate—is hardly ever sufficient. The fundamental testimony as to mental process must always be that of introspection, only when this latter has gone as far as feasible, should and must free scope be allowed to any procedure that is dominantly inferential.

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[1] See G. E. Müller, as quoted by C. S. Myers in his *Textbook of Experimental Psychology*, p. 136.

[2] *J. of Anim. Behav.* 1917, p. 142.

Now, to obtain reliable introspection from children is difficult under any conditions, and quite impossible when they are at the same time required to do a difficult performance as well as they can. Consequently, the introspection needed in this research had to be effected by other subjects, psychologists thoroughly trained in such work. To those who served me in this capacity, my hearty thanks are hereby tendered. They did the experiments in precisely the same manner as the children, except for the interruptions caused by their dictating what they observed about their mental processes. Such observations were obtained in considerable abundance. But for the present purpose, the following typical extracts would appear to be adequate, and, indeed, as much as our available space can allow.

1. *Introspector A.*

After the first run.

I found great difficulty on coordinating diagonal movements. I found it easier to deliberately make a mistake, and then correct it in the next move.

After the second run.

I anticipated difficult corners, and check the first impulse and try to go in the opposite direction.

After the third run.

I am beginning to guide myself by intentional directions. There is a distinct consciousness of the contrariety of my movements along with a knowledge of the mirror character of the percept.

2. *Introspector B.*

I first try to think how I ought to move, and then make the movement. I at once see the relation (of divergence) between the path I intended and the one I did actually move. I tried to direct intentionally the movements and found I moved in consequence deviating all the more. I often noticed that it carried me in a direction opposite to where I wanted to go. I therefore tried to think what would be the natural way to move, and then tried to move in a direction which is reverse to that. The adoption of such a procedure was sometimes successful (in the case of the vertical lines) and sometimes not (in the case of the diagonal lines).

3. *Inferences corroborated.*

On the whole, evidently, such introspections—obtained from adult psychologists of very high training—are fully in accord with the inferences made from the actual movements of the children. The conclusion appears legitimate, that the two classes of subjects really underwent very similar cognitive experiences; or, in other words, that the inferences made from the movements of the children were—if not in all detail, at any rate in general—sufficiently valid.

V. THEORIES.

1. '*Mechanical*' *theories.*

Let us now consider very briefly how the achievement of learning, or in other words the tendency of the movement to approach towards that which is intended, can best be explained.

Such explanatory theories fall into two groups, which have often been characterised as 'mechanical' and 'rational' respectively.

Among the former is that formulated by Perrin as follows:

> If the learner responds to his problem by a narrower series of habit-adjustments, the conclusion is suggested that each step in the process not only prepares the way for, but actually arouses the succeeding step....According to this interpretation it is not necessary to assume that reactions are 'discovered' by a trial and error process and that consciousness then selects the one most fitting. Rather the habit 'discovered' is the one brought by the stimuli and the one most likely to be needed. If this is true the chances are more favourable for its re-arousal and permanent selection.

Against this must be urged that to suggest habit as initiating *variation* in response is to make the term meaningless. For the essence of habit is to withstand change and conserve the old familiar form of reaction. If, on the other hand, he does not mean variation by the words 'succeeding step,' then Perrin's theory does not touch the most central fact about the learning.

Another of the mechanical theories is advanced by I. Peterson in the following physiological terms:

> When the basic life processes controlled by stimuli through the automatic nervous system becomes impeded in any manner, nerve impulses flow into various channels bringing about random incoordinated acts involving the skeletal muscles. Consummatory acts seem to spread their effects less extensively throughout the organism than do incoordinated acts....*The random impulses will be eliminated in the order of their proximity in neural relationship to the consummatory acts*[1].

Against this view may be urged the illegitimacy of assuming that the 'consummatory acts' were already present scattered among the 'random' ones, and had only need of surviving while the others were eliminated. In point of fact, the experiments indicated that the original acts were *never* consummatory, but always followed the coordination of normal, *i.e.* non-mirror, space. Further, what is meant by the mysterious term 'neural relationship'? And *why* should this cause any such elimination?

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[1] *J. of Exper. Psychol.* 1922, p. 267.

In the preceding theory we have come upon a notion which plays a large part in almost all these theories of the 'mechanical' sort; this is the notion expressed in such terms as 'random' or 'chance' movement. Now, a chance event has been aptly defined by Udny Yule as one "due to an extremely complex system of causes of the general nature of which we are aware, but of the detailed operation of which we are ignorant[1]." In a typical case of learning like that of the mirror drawing, what factors may fairly be said to be 'chance' in the sense just mentioned? On the purely physiological side, there are changes in the composition of the blood due to the formation of toxins, fluctuations in the secretions of glands connected with the sympathetic nervous system, and so forth. On the psycho-physiological side, there are changes in the postural tonus, with reference to which alone movements can be recognised, and there are alterations in the state of fatigue or facilitation of the neuro-muscular system involved in any work. None of these seems in the least degree to account for such variations of movement as we have actually observed.

2. *'Rational' theories.*

Usually opposed to the theories of 'mechanical' procedure, which we have just had to reject, is that of 'rational' or 'intelligent' action. And towards this latter alternative it is that our experimental results would appear to point.

In Exp. 1 the success of the different children correlated with their intelligence as estimated by their teachers to about ·81 m. (p. 281), whereas in Exp. 2 the correlation turned out to be only ·05 (p. 285).

Here, however, the thorny question arises, as to what precisely the terms 'rational' or 'intelligent' are really intended to signify. The definitions most widely current show themselves in the present case to be of little or no use. Thus, if we take the classical meaning of the terms, we must expect some sort of syllogistic operation; and our present results, both objective (p. 284) and subjective (p. 285), fail to indicate anything of the sort. If, on the other hand, we turn to the nowadays much-favoured defining of intelligent behaviour as 'adaptation to a new situation,' this certainly occurs in the operation of mirror drawing in whatever way it may be accomplished; the said definition, then, contributes nothing at all towards deciding between the two rival theories as to *how* the accomplishment takes place.

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[1] *Introduction to Statistics*, p. 30.

More satisfaction can be obtained by turning to the analysis into six ultimate processes, which has been mentioned already (p. 278) and which claims to comprehend all cognitive operations whatever. That claim, so far as the present experiments are concerned, would appear to be corroborated. For in these, both the objective and the subjective results have led invariably and unequivocally to just these ultimate processes. Three of the latter have been especially conspicuous (the *A*, *B* and *C* formulated on p. 278). One of these, *C*, or associative reproduction, alone merits the name of 'mechanical'; it dominates the commencement of the learning, in the shape of habit established by life-long usage; in lesser measure, it dominates also the end of the learning, as habit established in the course of the whole operation itself. The other two conspicuous processes were *A* and *B*, those of educing relations and correlates respectively. These play their chief part in the central portion of the learning (p. 281). They are of the species that have been called 'noegenetic.' They may therefore be called intelligent, *if* we agree to define intelligence as the power of noegenesis.

3. *Corollaries, theoretical and practical.*

There still remains a very important question. Since the kind of learning involved in the present experiments is to this degree 'intelligent,' how then shall it be distinguished from the kind that commonly earns the latter name? For assuredly, there exist between the two kinds some large differences. In particular, the present learning occurs very gradually, as does all such procedure currently termed that of trial and error; whereas the other learning occurs by sudden bounds, which appear to indicate an origin in flashes of intellectual illumination.

The answer would appear to be that the trial and error and the intelligent learnings do indeed resemble one another in so far as both alike rely upon the processes of educing relations and correlates. But they differ from one another in that the trial and error operation is chiefly effected by conscious processes of low intensity, high speed, and wide distribution, whereas the intelligent operation is the reverse in all respects; the former may be called marginal, and the latter focal.

This comparison between the two kinds of learning appears to be suggestive of many important consequences, both theoretical and practical. To begin with, the often asserted trenchant line between the two now fades away into a perfectly continuous transition; marginality and focality are merely matters of degree. This standpoint appears to shed much light upon and to impart a new unity to the psychology of children and that of animals.

Again, since the two learnings do not really differ in form, the lower or marginal kind admits of ready conversion into the higher or focal. To do this, we need only analyse the required operation correctly into its ultimate processes as described above, and then explain these processes to the learner. On so doing, moreover, the whole learning—here in mirror drawing, at any rate—would appear to become greatly facilitated. If hitherto attempts at conversions of such a nature have met with but sorry success, this has perhaps been due to want of the indispensable preliminary, a correct analysis of the motor operations into their ultimate processes. Now that this analysis is available, a vista is perhaps opened of new possibilities for training in manual skill.

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IMAGERY AND MENTALITY[1]

By T. H. PEAR.

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I. INTRODUCTORY.

THE opinion which this paper will express, and attempt to substantiate, is that the extended psychological interest in the study of the behaviour of man is being accompanied by neglect of certain vitally important considerations arising out of the structure of mind itself. The necessity for applied psychology to make frequent returns to consider questions of general theory will be illustrated by certain considerations arising out of the neglect of mental imagery.

II. THE NEGLECT OF IMAGERY-DIFFERENCES.

In a recent paper on "Vehicles and Routes of Thought[2]," I have attempted to illustrate the importance to the psychologist of continually bearing in mind the fact that in different persons the thought-processes may be carried by different vehicles (sensations, images, conscious attitudes and the like). Moreover, the thinking may proceed by different routes, even when its ultimate expression, in words or actions, is similar in different people[3]. The extreme behaviourist appears to avoid both these considerations. But there are others who neglect it; which in practice, though not in theory, comes to the same thing.

For this neglect an apparent excuse might appear in the suspicion with which psychologists are apt to approach any rigid doctrine of *types*. And the early belief in types of imagery has produced in some quarters a natural reaction which appears to have gone too far. Investigation failed to find a pure type, and shattered the hopes of those who believed that some day children might be divided into classes of audiles, visiles and the rest, and taught by duly selected visile and audile teachers. But the denial that pure types exist seems like the declaration that Dickens characters are not met with in real life. Both assertions may be scientifically and theoretically true, and both are practically and artistically misleading. For there are people who in most of their dealings with the world of thought employ, or are employed by, predominantly one kind of imagery. And for practical purposes they *are* types.

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[1] The substance of a paper read on Sept. 13th, 1923, before the Psychology Section of the British Association for the Advancement of Science, Liverpool.

[2] *Discovery*, IV, August 1923, pp. 203–6.

[3] Cf. "Is Thinking Merely the Action of Language Mechanisms?" This *Journal*, 1921, XI, pp. 72–80.

In this article, therefore, it will be convenient to use the old-fashioned word 'visile' to designate, not a person who is incapable of any imagery other than visual, but one in whose dealings with reality visual imagery plays the predominant part. Similarly, the motile will mean a person whose muscles, other than those controlling speech, dominate his thinking. For the person who thinks predominantly in words, the term verbaliser will be used[1].

These words will be sufficient to mark out the types to which the present article refers. They are unsatisfactory, are employed only for the present purpose, and should be replaced by others, when they can be found.

It is well known that one cannot confidently predict from the behaviour of a person what his predominant type of imagery will be. A successful artist, for example, need not be a visile, or a musician an audile. Moreover, a person gifted with a particular type of imagery may perversely use another for every-day affairs. Yet I believe that these are extreme cases, interesting because of their unusualness; that undue importance is being attached to them; that the mentality of most ordinary persons is seriously affected by their predominant imagery, and that at present there is a fairly general tendency in psychology to neglect this important consideration. Furthermore, some persons, placidly accepting the unfortunate fact that many of their own kind glut their eyes and starve their ears and muscles, write as if everybody is either a visile or ought to be one.

These tendencies can be clearly seen in recent writers. One, in reviewing a book which professed to deal with remembering, criticised it as being chiefly not about remembering, but about imagery. Now, a complaint that an author who professed to write about travel had produced only a book on railway trains might be justifiable to-day, for in civilised countries these vehicles have now reached a state of comparative safeness, predictableness and uninteresting similarity. But few would object if a book which professed to deal with flying devoted a fair amount of space to the differences between the outstanding types of airships, aeroplanes, gliders and parachutes. The reason for this is obvious; in travelling by air one cannot yet be certain of the type of machine which will be available, of its behaviour, of its landing-place, even of its mode of descent. Similarly, we do not yet know enough about the structural aspect of thinking to justify our taking its vehicles for granted. If we are so foolish, we shall pay for it by believing that the vehicles and routes of a person's thoughts are known to us when he reached his conclusions in quite another way and on a different type of machine.

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[1] Cf. F. C. Bartlett, "The Functions of Images." This *Journal*, 1921, XI, pp. 320–337.

This is after all, a sin of omission. But we now pass to a sin of commission; a way of regarding imagery which is so unpsychological that it must be mentioned here, with the fundamental objections to it. It is exemplified by Prof. Lewis M. Terman, when, in his useful book, *The Measurement of Intelligence*[1], he writes:

> Thought may be carried on almost equally well by various kinds of imagery. As Galton showed, a person can be taught to carry on arithmetical processes by the use of smell imagery.

An obvious comment upon this is that the second sentence does little to support the first. The first, unless we interpret the 'almost' so leniently as to destroy its value, seems to deny a fact of experience. It involves the improbable assumption that one could think about a painting almost as well as if one had no visual imagery, or think about music almost as well as if one had no auditory imagery. The present writer, at least, can assure Prof. Terman that, having practically no auditory imagery, any real thinking about *music*, as distinguished from its notation, its physics, its associations and other attributes which can be expressed in pictorial terms, is almost impossible for him.

Prof. Terman continues:

> We may say that imagery is to thinking what scaffolding is to architecture. The important thing is the completed building rather than the nature of the scaffolding employed in erecting it. No one thinks of blaming the ill-construction of a building upon the kind of scaffolding used, for if the architect and builder are competent satisfactory scaffolding will be found.

One could wish that a brother psychologist might have been present to ask Prof. Terman, immediately after he wrote this statement, to specify the kind of imagery to which he was indebted for it. In any case, the paragraph seems excellent evidence that thought cannot be carried on *equally* well by various kinds of imagery. For surely, if there is any resemblance between imagery and scaffolding, it does not extend very far. A scaffolding is made of characterless lengths of homogeneous wood, most of which can be placed indifferently almost anywhere. An image is a whole a growing or shrinking, but living entity; the outcome, perhaps, of the mind's inveterate tendency to make whole things, a tendency displayed also in perception, in testimony, in secondary elaboration, in the special difficulty of giving a true account of anything incomplete. Many things can be made with a Meccano set, but not an image.

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[1] London, 1919, pp. 328–9.

Nor can you dispose of an image, when it has helped you to build up a thought, a belief, or a whole attitude towards life, so easily as you may disintegrate a scaffolding and store it in bits to perform a different function on another day. Your image may disappear from waking life only to come back in dreams or fever; the bond linking the memories of early childhood to their supporting images may be indissoluble and insistently recurrent. Moreover, when the mind, faced with unusual difficulties, slips into a lower level of operation, back come the images and symbols from some limbo which nobody has yet described satisfactorily, but which is very real.

If imagery be a scaffolding, it is a queer one. For it is alive, changing, dominating, the alternate delight and despair of the contractor and the architect, leaving the impress of its own anatomy on the building, and often hard to get rid of when the job is done. The truth is rather that the apprehension of any meaning, which has developed by means of the support of a particular kind of imagery, is apt, throughout life, to be shaped along the lines of that first imagery. Moreover, it is subsequently extensible through the medium of this original kind of imagery more easily than through another. Hence the popularity in our own time, of the illustrated paper and the cartoon, and in this country, of politicians' references to hen-roosts, steam-rollers, tunnels and sunrises.

That in some minds meaning may eventually function in complete, or almost complete independence of the type of imagery which originally carried it, is, I believe, a probability. But this appears to be true rather of the person who has been trained to think generally and abstractly, and we must not mistake the occasional attainment of an ideal for normal or average behaviour.

All the evidence seems to support the belief that the thoughts of ordinary persons usually travel by the vehicles to which they have been accustomed, and that, on this account, we must often interpret statements which appear to be generally valid, as applying to their makers, and to others of similar mentality, but not necessarily to all minds in their vicinity.

Examples may be taken from the writings of modern psychologists.

The first illustrates the tendency of a writer on psychology to assume that other minds are like his own.

Dr John B. Watson, in his books *Behavior* and *Psychology from the Standpoint of a Behaviorist*, describes the conduct of human beings in certain situations in a way which might appear to be generally true to any reader whose predominant remembering apparatus happens to resemble Dr Watson's. I will not commit the error of trying to diagnose Dr Watson's mentality from his writings, but merely record here the impression that kinaesthesis, actual or imagined, is prominent in his thinking, and that his acquaintance with what a visualiser might call a sturdy, robust visual image appears to be slight. To a predominant visualiser, Dr Watson's account, though interesting, might seem to relate to an inhabitant of another planet, as presumably the visile's might to Dr Watson.

Dr Watson says that when we watch a fight we "tend to ward off a blow or strike a blow"[1], that when in his study he tears off his calendar leaf, he says aloud "tear off leaf," and that if he *thinks* the words "tear off leaf," "the words must be uttered silently before the habitual act arises[2]." But there are visualisers who can watch fights or any other violent human activity almost, if not quite, as if they were seeing moving pictures; who feel little or no tendency to copy the action in their muscles, deeply regretting this fact when they take lessons on games; and who are bored to the utmost degree of stiffness by watching athletic sports, or other muscular displays unless they be exciting or beautiful. Such persons would be very surprised if they found themselves muttering commands to themselves, regarding such a liberty on the part of their speech muscles as permissible only in moments of great emotional strain. They are inclined to assume towards a book on human behaviour written by a motile the incredulous attitude that might be adopted by literate birds towards a zoological treatise from the pen of a cultured mole.

But there is another side to this question; indeed, the problem is polyhedral, possessing as many facets as there are persons with strongly predominant types of imagery. We will, however, confine our remarks to the visile; the type of person who is most likely to be critical of writers like Dr Watson.

In this world, where eyes and things seen have been brought to such a pitch of perfection, the visile enjoys certain advantages, some of them, possibly, undeserved. Poets have conspired to cushion life for him; a fascinating theme which we cannot pursue here. Our immediate aim is to inquire whether his skewed interpretation of the world may not lead us astray if it tinctures his psychological descriptions. Some of these descriptions appear to assume, or at least to postulate, that primitive thought-processes—and, indeed, thought-processes several stages more complex than this—must of necessity be visual in their nature.

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[1] *Psychology*, p. 108.

[2] *Behavior*, p. 333.

This assumption may prove to be justified, but nevertheless, it is an assumption. To question it involves lifting the lid off a tangled set of problems, from which the following may be separated without much difficulty:

(1) Is primitive thinking of a visual nature, (*a*) in the human race in general, (*b*) in the early life of the individual, (*c*) in the adult life of the individual?

(2) When, as in dreams, delirium or hallucination, awareness becomes predominantly visual, is this primitive thinking?

(3) When the results of psychological analysis cause another person to deem that the patient has had unconscious thoughts, what justification is there for assuming that, while unconscious, they were carried by visual imagery?

(4) Are we justified in assuming that, because when the results of unconscious thinking (if thinking it be) are revealed to consciousness, they appear in a visual form, unconscious thinking is visual in nature?

(5) What kind of primitive thinking may be assumed in persons congenitally blind, who, *ipso facto*, can have no visual images at all?

Thoughts such as these are aroused by reading Dr J. T. MacCurdy's interesting contribution to the recent International Congress of Psychology; a paper on "Instincts and Images." It is so strikingly different from Dr Watson's account of behaviour that at first the visile may forget to criticise, as a wine merchant's tender sympathies might be extended towards certain writings by Mr Belloc and Mr Chesterton. Yet it may fairly be asked whether, since the visile does not approve of Dr Watson's account, because, among other drawbacks, it does not describe the visile's peculiar type of mentality, he begins with a prejudice in favour of Dr MacCurdy's because it does. The visile who reads this account of how intelligent minds work, should therefore bear in mind that it has been written by a visile[1]. And he should certainly consult any motiles he can find; certain 'verbalisers' or verbal thinkers—especially those who have grasped the fact that it takes a visualiser to understand a visualiser; and—most important of all—those thinkers who declare that in their thought-processes images are very rare.

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[1] I have Dr MacCurdy's assent to this statement.

Dr MacCurdy describes "the probable mental operations of Pasteur, when he separated the crystals of racemic acid into two groups, one of which gave a dextro-rotary, and the other a laevo-rotary solution."

He asks what constituted, psychologically, Pasteur's faith in the crystallographic method. His answer runs:

> One obvious element is prevision of success; he sees imaginary differences among the crystals of racemic acid, and this image is so persistent that naked eye inspection of the real crystals does not convince him of the differences being absent. He goes on looking until the inner image and an outer perception are made to agree.

Now it should not be forgotten that Dr MacCurdy has written of *images*, and has not specifically stated that they are visual. In theory his statements may be translatable into the imagery of other senses. Yet Pasteur is assumed, in later sentences, to be a visualiser. Dr MacCurdy supposes Pasteur to "*see* imaginary differences," to "go on looking" until the image and the perception are made to agree; in short, he assumes (*a*) that Pasteur's discovery rested upon the utilisation of imagery, and (*b*) that these images were visual in nature.

It seems reasonable to doubt the value to psychology of such structural dissections, not only *post-mortem* but *in absentia*, and to believe that it is more profitable to ascertain the mental processes which actuate scientific discoverers who are alive. Such a task is quite difficult enough. The psychological study of brilliant thinkers is likely to prove of great value when the method of question and answer is practicable. But to return to Pasteur. It seems very possible that he may have used such imagery, and, as Dr MacCurdy rightly reminds me, Pasteur did look down a microscope. But he might conceivably have employed words, with or without accompanying muscular imagery and sensation (as for example, in tendencies to illustrate rotation) or even imageless thoughts.

Nothing is further from my mind than to put unnecessary difficulties in the way of a determined attempt to account for intelligent thinking. And to work with images seems modest and unobjectionable as a beginning. The proof that Pasteur's discovery really was made in this way would be a feather in the cap of the imaginal thinker, and might raise his status in the minds of philosophers. But that proof is improbable. Dr MacCurdy's paper, therefore, appeals to me for such special reasons that I greatly desire to read a reasoned judgment upon it by non-visiles. In any case, since visualisation appears to be the commonest form of remembering, his explanations may prove to be true of a large number of persons.

III. IMAGERY-DIFFERENCES AND PSYCHO-THERAPY.

It is possible to bring evidence in support of the belief that psychotherapy would gain by paying more attention to the imagery characterising different mentalities. The following example illustrates this:

An intelligent patient, during the course of a severe nervous trouble, was greatly troubled by pseudo-hallucinations of a distressing kind, which vanished after their sources had been traced by analysis. During an interview with the doctor, the patient complained that, although on leaving the consulting-room he invariably felt better for an hour or so, after that time the meaning of the conversation which had relieved him entirely vanished, and the time seemed to have been wasted. One day a colleague suggested that this might be connected with the patient's predominant visual imagery, and that therefore it might be worth while to try the effect of writing down for him the gist of the hour's conversation. This was done. The patient then reported that whenever one of these specific worries arose he would read the slip of paper which summarised the therapeutic conversation about it, and feel immensely relieved. One night the doctor had attempted to lessen the patient's serious anxieties by a simple explanation of the brain's functions, illustrating his remarks by a sketch of the cerebrum. When, that night, the hallucination appeared in the darkness, there spread over the centre of it an image of the brain-diagram, which reassured him and brought him great comfort.

Though this is an extreme case, it illustrates the way in which spoken words may fail to affect certain individuals, not because the words are not understood, but simply because their appeal is in a medium which is under-developed in these particular persons[1].

Among the conflicting accounts of the processes which are supposed to occur in the process of auto-suggestion, at least in that form of it which is recommended by M. Coué, I do not remember seeing the hypothesis that its varying success may be due in part to the type of imagery from which the subject, so to speak, launches his new, and for him, difficult action. Does auto-suggestion appeal to the habitual verbaliser, so that the gabbled command is merely strengthening by repetition, and by the emotional intensity of the experiences, the usual mechanism which launches action after deliberation? Or is it the person who seldom talks to himself, who adopting this unusual behaviour under the hetero-suggestion of the physician, gains control over himself by a new path? This is an interesting question for theory but an important one for practice. For the different mentality of those who find auto-suggestion successful or the reverse may account for its varied success.

How far, again, may that implicit recognition of the importance of the outer world, a certain amount of which is necessary for the most absent-minded if they are to maintain relations with it, be connected with kinaesthetic imagery? And, further, which is cause and which effect? Or are they both developments of early childhood, in which the really potent factors are those intimate human relationships with which the psycho-analysts are dealing? Imagine a boy brought up with a dominating father or schoolmaster; one who temptingly puts in his way at every moment games, athletics, handwork, social activities and responsibilities, and supports these influences by his own example. It would not be surprising if the boy became a typical extrovert. Imagine him brought up with wealthy dreamy parents who appreciate art, music and literature but do not practise any of them. If he develops a particular class of imagery and becomes an extreme introvert, how much was due to nature and how much to nurture: and did the imagery provoke the behaviour, the behaviour mould the imagery, or both?

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[1] *Discovery: loc. cit.*

Or again, passing from matters of etiology and diagnosis to therapy, what of the ergotherapist's creed; the 'work cure'? May it be that the enthusiastic ergotherapist is a motile, or a person, painfully conscious of his own unusual motile deficiencies, who has overcome them? But this would lead us from the psychology of psycho-therapy to the psychology of psycho-therapists.

(*Manuscript received* 13 *October*, 1923.)

A NOTE ON THE PSYCHOLOGICAL SIGNIFICANCE OF THE PSYCHO-GALVANIC REACTION

By HONORIA M. WELLS.

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THE point with which this note deals emerged as a side issue during a research on 'choice,' and appeared to be of sufficient interest to warrant special investigation. Details of the method of research cannot be given here. It must be sufficient to state that the subjects had to make acts of voluntary choice at a given moment; that records of changes in skin resistance and respiration were obtained covering the reaction period; and that introspections were immediately dictated. By this means we hoped it might be possible to correlate specific factors reported in the introspection with any significant changes in the records.

On examining the photographic records of the subject's psycho-galvanic reflex, it was noted—allowance being made for the latent period—that a fall in resistance occurred[1] with great regularity at the time of choice in the case of every subject—7—tested. The actual deflection measured on the record was in each case expressed as a percentage fall on the original resistance; and the average percentage fall in resistance at the period of choice was worked out for four of the subjects[2].

Table I. *Average percentage drop in resistance at choice.*

Subject	No. of choices	Percentage drop
A	180	2·75
B	71	6 38
C	57	4·03
D	39	6·34

Choices were then differentiated according to their nature; the criterion in the selection being the subject's introspective report of his experience during the choice. Cases of 'difficult' or 'hard' choice were grouped together. These included a large proportion of cases of bad adaptation and of negative motivation—the alternatives often being judged as equally good, bad or indifferent to the subject. In these cases the subjects report that they had to make a more than ordinary effort to bring about the choice at all; *i.e.* to strengthen one of the motives to the 'Critical Point[1].' We found, with every subject examined, that the fall in resistance was proportionally greater in these cases of difficult choice[2].

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[1] Cf. Fig. 1.

[2] Cf. Table I.

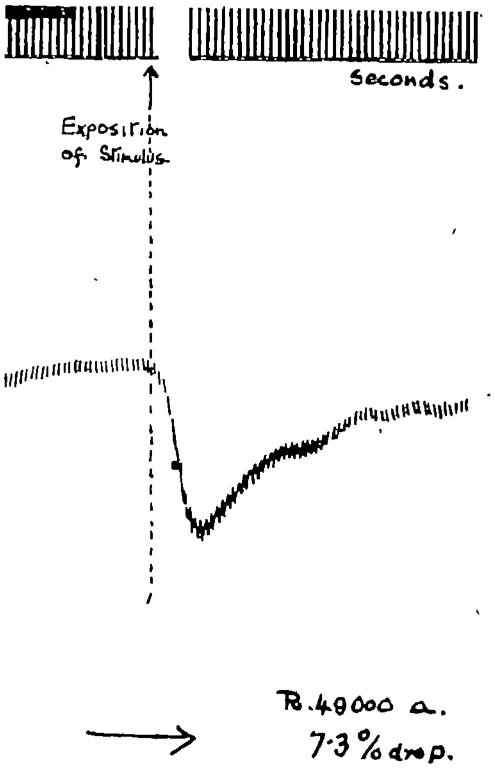

Fig. 1.

Table II. *Comparison of average percentage drop in resistance in 'difficult' choices, with average percentage drop.*

Subject	I Av. percentage drop for all choices	II Av. percentage drop for 'difficult' choices	Variation of Av. II from Av. I
A	2·75	3·77	+37·1 %
B	6·38	11·70	+83·3 %
C	4 03	6·52	+61·2 %
D	6·34	8·88	+39·9 %

According to the generally accepted theory the reflex is indicative of emotional processes. This appears to be the view adopted since its original formulation by Féré. The fact that few, if any, processes of an emotional character were reported in our choices, led us to an examination of the protocols with a view to discovering other processes to which the deflections occurring synchronously with the choice (allowance being made for the latent period) might possibly be related.

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[1] Cf. Boyd-Barrett, *Motive Force and Motivation Tracks.*

[2] Cf. Table II.

All the subjects at some point in the course of the experiments experienced a marked change in the character of consciousness at the moment of choice. They made great efforts to describe this; but it proved very elusive and difficult to characterise. The following are some of their attempts:

"An intentional turning to Laip[1]"; "Distinct turning away"; "A sort of swing of the mind"; "A mental direction to Klum"; "An intentional turning to Ziv, faintly tinged with action. There were also muscular feelings throughout from eyes and forehead"; "Choosing Mep seemed like aiming a dart to hit it"; "I seemed to jump upon the alternative"; "As if I were being thrown in the arc of a circle, but I'm sure my eyes did not move"; "Sort of mental movement to Tauk"; "Swing of attention with feeling of activity"; "Mep chosen after a blank; nothing present in consciousness but a lot going on"; "Mind swung back"; "I seemed very active"; "A mental rebound"; "Seemed some alertness at the choice"; "Feeling of mental strain; great activity, but it seemed pent up."

Two of the subjects, familiar with previous research on volition, after a few attempts to find expressions of their own for this phenomenon, definitely identified it with the 'Consciousness of Action' reported by Michotte; and all the subjects agree in describing it as a dynamic state accompanying direction of attention. When it appears, there is an abrupt transition in the course of consciousness; all the cognitive elements of deliberation seeming to drop away, a wave of conational effort surging up, and the experience of 'action' filling consciousness to the relative exclusion of everything else.

The following introspective expressions indicate this:

"There was a blank—or, rather, an abrupt cessation of the previous processes, and a total break in the direction of consciousness"; "There seemed to be a considerable blank"; "Conscious of nothing but this mental effort."

Sometimes this phenomenon is fused with feelings of muscular strain which may complicate, or even 'mask' it; but, in the course of the experiments, all subjects were able to isolate the muscular feelings, tensions, etc. and state that they can make a quite clear distinction between effort of this sort and the consciousness of action.

"If there was a consciousness of action, I think it was masked by the movement of the hand"; "Seemed a mental movement"; "Tension from eyes, neck, forehead, which almost masked the consciousness of action"; "A mental and physical effort";

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[1] The experiments involved the use of nonsense syllabus.

"Feeling of alertness present. Noticed also tightening of upper eyelids, and some physiological sensation I could not localise"; "In the feeling of alertness a *qui vive* is left if we take away the physiological sensations accompanying it"; "A gripping of the self, with opening of eyes"; "Bodily swing very clear, mental swing of a vague nature"; "Feeling of activity seems to grow and then get small again, and with it is the feeling I am stretching my eyes." One subject thus explains his distinction between alertness and excitement: "Alertness is feeling prepared to grasp the stimulus and deal with the situation. The excitement seems a bodily state-breathing, etc.".

The point to be emphasised in this connexion is that all the expressions chosen by the subjects to describe the phenomenon in question are essentially conative in character. This gave us a clue to the view, for which evidence is put forward in the present note, that the psycho-galvanic reflex appears as an indication psychologically of a conative rather than of an emotional state. With a view to throwing more light upon the apparent parallel between the psycho-galvanic reflex and conative effort in the act of choice, an attempt was made to discover the presence or absence of a quantitative relationship between them. Three of the subjects, unsolicited, discriminated such quantitative aspect in their experience of 'activity,' and reported various degrees of consciousness of action, as strong, distinct, moderate, weak, faint, tinge. Others, on being pressed, were able to make some estimate of it. The average percentage drop in resistance in choices reported as 'automatic,' 'habitual' or 'easy' was compared with that of choices in which strong consciousness of action was mentioned. The following results were obtained from data provided by a subject who characterised consciousness of action in quite unambiguous terms.

Table III. *Comparison of average percentage drop in resistance for different types of choice, for subject A.*

I	Av. percentage drop for all choices	2·75
II	Av. percentage drop for 'automatic' choices	1·76
	Variation of Av. II from Av. I	−35 5 %
III	Av. percentage drop for choices where subject reported strong 'Consciousness of Action'	3·3
	Variation of Av. III from Av. I	+26·6 %

In the case of other subjects examined up to the time of the writing of this note[1], it has been somewhat difficult to classify the choices on the same basis, owing to their inability to isolate consciousness of action during the early part of the research, and to find terms in which to characterise it. 'Automatic,' 'habitual' or 'easy' choices are easy to isolate in the protocols of all the subjects; but, in order to classify choices corresponding to those in Table III, column 3, it has often been necessary to infer the presence of the phenomenon from the expressions used by the subjects. All, however, agreed, after the completion of the experiments, that they had all been endeavouring to describe consciousness of action, and that choices where it appeared in comparison with those described as easy or habitual are more difficult to make; that it was necessary to make more effort to choose; that there seemed to be a greater output of energy.

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[1] Cf. Table IV.

Classification on this basis gives us the following results:

Table IV. *Comparison of average percentage drop in resistance for different types of choice, for all subjects.*

	I	II		III	
Subject	Av. percentage drop for all choices	Av. percentage drop for 'automatic' choices	Variation of Av. II from Av. I	Percentage drop for choices where 'Consciousness of Action' was inferred	Variation of Av. III from Av. I
A	2·75	1·76	−35·5 %	3·3	+26·6 %
B	6·38	2·48	−61·0 %	8·85	+37·1 %
C	4·03	2·15	−53·0 %	6·4	+58·0 %
D	6·34	3·9	−38·4 %	8·45	+33·1 %

Later investigation revealed the fact that this distinction between these two kinds of choice does not provide a sound basis for classification. This led us to the point of view that a distinction might have to be made between the highest type of voluntary choice and a dynamic state which is not volitional. This dynamic state is the consciousness of action; and we believe it to be present in every type of choice.

The following are reported in the protocols:

"Choice automatic, yet active"; "Active, but not volitional"; "Am inclined to think that this action factor is present in what I have called automatic choice"; "I think one is continually having consciousness of action."

Normally we fail to isolate it as an item of cognised-experience; but where the choice becomes impeded or difficult, consciousness of action generally becomes an observable phenomenon. As was mentioned previously, most subjects came to discriminate a quantitative aspect in it; and attempt to report the degree of intensity of activity. We may then regard the condition for its appearance in consciousness as the intensification of a conative state.

From this part of the investigation we can formulate the following general conclusions. Numerical results give us evidence for the belief that there may be some relation between conative states and the appearance of the reflex. Where a greater drop in resistance occurs, we have introspective evidence of an intensification of the conative state. As emotional processes reported were few or none, we may formulate the hypothesis that *the occurrence of the reflex, whatever it may signify with regard to physiological change, is regularly preceded by changes in consciousness of a conative, rather than an emotional, character.*

On examining some 550 photographs of the reflex, it was seen that, besides the deflection at the choice period, two other characteristic deflections invariably occurred at given points in the record. During the experimentation it was necessary to give, from the experimenter's room, a signal to the assistant operating the kymograph, as well as one to warn the subject of the approaching stimulus. Between these, the Hipp chronoscope was started; and its high, singing note was heard by the subject. It was noticed that two deflections occurred, besides that of the choice. Allowing for the variable latent periods of the reflex, these falls in resistance were found to synchronise with the time of the signal and the clock[1]. The average percentage fall in resistance for the warning signal and the sound of the Hipp was calculated for each subject. The results are given in Table V.

Table V. *Average percentage drop in resistance at the signals.*

	I	II	
Subject	Percentage drop at first signal	Percentage drop at sound of clock	Percentage drop at choice
A	1·07	·63	2·75
B	3·2	1·8	6·38
C	2·5	·9	4·03
D	3·7	3·0	6·34

NOTE.—All cases of the reflex at the signals have been included in Cols. I and II. They include a large number of cases where the deflection was very small or failed to appear; and where in the introspection the subject reports not having heard or attended to the signal. These tend to lower the average considerably.

Accordingly, an attempt was made to analyse the mental processes occurring at these points with a view to finding corroboration of the hypothesis previously stated. Sometimes these were forgotten by the time the subject dictated his introspection on the period of choice; but they were, on the whole, remembered with astonishing completeness. Several processes were discriminated which seemed of interest.

The usual phenomena of expectant attention occurred—muscular sensations in chest, neck, jaws and eyes; strong tactual pressure sensations of the tongue against the lower teeth; a vaguely undifferentiated sensation designated as strain. But, as a rule, consciousness was so contracted at these points that little was in focus other than the vague strain.

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[1] Cf. Fig. 2.

Thoughts, or mental directions towards what would appear, were not infrequent. No affective processes were reported.

What was most salient was a process which could not be satisfactorily named by the subjects. The first to be investigated spoke of 'excitement' and 'pulses of consciousness.' 'Swellings' were also spoken of in this connexion, and were related to eye-movements, especially of the left eye. At length 'alertness' was chosen as a more or less suitable term, though it was recognised that it was not precisely what was wanted. The subject noted the similarity between this state of alertness and consciousness of action in the choices; and was very insistent in emphasising the purely mental character of the phenomenon, and in distinguishing it clearly from muscular sensation, strain and effort which might or might not accompany it.

"This consciousness of action is of the same character or similar to alertness, but much more faint"; "Absolutely mental, not sensorial"; "Alertness is conative, and so is consciousness of action"; "Can discriminate same sort of phenomenon in designation of the alternative."

Other subjects found the same difficulty in discriminating this dynamic state experienced at the warnings. All finally insist upon its mental character, and differentiate it from felt muscular tensions, strains and visceral changes. Only a few of their expressions can be given in the space of this paper, but sufficient to indicate their fundamental similarity.

"Felt some sort of activity going on, could not catch what it was"; "The feeling seemed to fill consciousness"; "Nothing present to consciousness, but a lot going on"; "Seemed a mental movement, distinct from my bodily movement"; "A kind of mental jump"; "A feeling of anxiety, expectation, waiting"; "A tightening attitude"; "A gripping of the self"; "Experienced something which pulled me together suddenly."

This clearly indicates that the predominant processes occurring in consciousness at the warning signals are conative; and the results in Table V show that a characteristic fall in resistance constantly accompanies them.

What was remarkable in regard to this, however, was the fact that subjects shortly began to report on the relative strength of alertness for each of the three occurrences of it in the same reaction. Thus it was found possible to check their quantitative estimate by the photographic records of the deflections at each signal. The subject was asked to note the alertness at the first signal and to compare the intensity of the experience at the other signals—greater, equal or less—relatively to this.

Comparison of the subject's estimates with the objective fall in resistance revealed a marked correspondence. These estimates were made in 316 cases: subject A yields 119 correct estimates out of 142 attempts, *i.e.* 83·8 per cent.; subject B, 72 out of 96, giving 75 per cent.; subject C, 51 out of 78, *i.e.* 65·4 per cent. In total, of 316 cases 242 corroborated the reports. Where the subject reports great or marked alertness, the fall in resistance is proportionately greater than the average of fall for that particular signal; when little or no alertness, there is, most constantly, very little drop in resistance. What seems particularly significant is the fact that in cases where the subject reports not having heard or noticed the signals, there is little, if any, fall; and, where he is directed to pay careful attention to them, there is a noticeable increase in it. Indeed, so marked was the correspondence of the introspective report with the deflections, that the experimenter, who was photographing and watching the movements of the galvanometer light in another room, was able to predict to herself the kind of estimate that would be dictated.

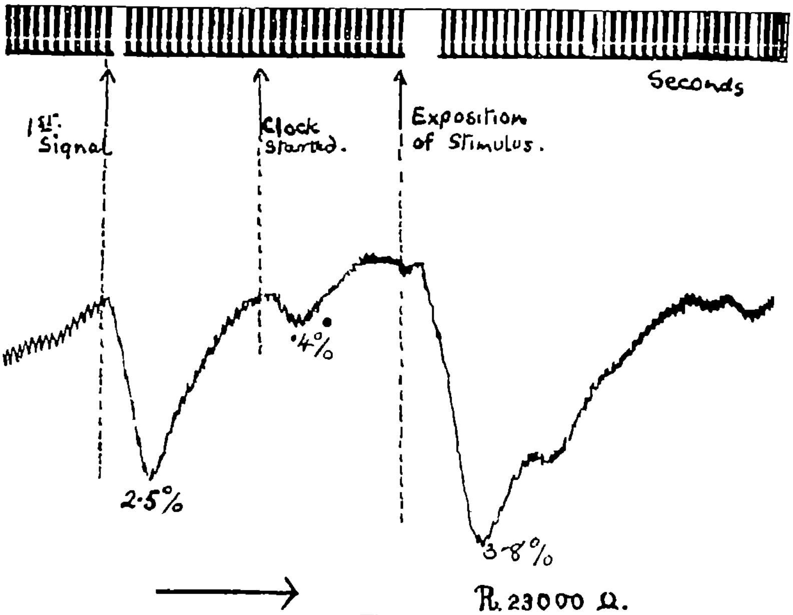

Fig. 2.

There seems sufficient evidence here to corroborate the hypothesis formulated earlier in this note. If the hypothesis is true, we should expect deflections wherever there is a release, or partial release, of the subject's latent conative energy. And this seems to occur at the exhibition of the stimulus, and at the signals; which latter can become neglected, with consequent failure in the reflex. Accidental occurrences in experimental procedure bear this out. Any departure from the usual course, claiming the subject's attention, is accompanied by a greatly increased fall in resistance. Many examples of this might be given.

Records were taken of the deflections: occasioned by 'noxious' stimuli, by the process of adding in pairs, the presentation of pleasant sounds, tastes and odours; some with practised subjects, others with children. We find no significant differences between the deflections for the various kinds of mental operations. In all cases a fall in resistance occurred just after the presentation of the stimulus. What appeared most constantly on introspection was what may be termed the conscious conative effort to perceive this—the direction of 'attention' towards it (as distinct from physiological effort).

And this is theoretically sound; since, before the stimulus arousing any emotional state can be cognised, there must first occur the conative state involved in some direction of the subject's attention towards it[1]. It is, of course, not denied that other mental processes may also develop; for cognitional sequences are certainly very frequently, and affective at least sometimes, reported in connexion with the perception of these stimuli. But their presence does not seem to invalidate our conclusion. Especially would this seem to be true in view of the fact that the process in question precedes the somatic resonance temporarily by at least some part of the latent period.

In the 'choices,' the great majority of the reactions were over and done with, including consciousness of action, before the full effect of the physiological changes could have entered consciousness as an 'emotion.' In the 'alertness,' the mental tension had become relaxed long before the latent period had elapsed. Only on the supposition that the physiological change, as such, *is* the emotion, and not that which appears to consciousness *as* the emotion, could it be psychologically maintained that the psycho-galvanic reflex is caused by the experience of emotions. And we may, therefore, conclude—since stimuli to which we pay no attention altogether fail to evoke it—that the regular conscious antecedent to this physico-physiological phenomenon is of a conational nature.

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[1] We prescind here from the general question as to whether emotional states can occur without cognitional antecedents. In our experiments, the significant reflexes were always dependent upon cognition.

(*Manuscript received* 17 *November*, 1923.)

CRITICAL NOTICE

An Outline of Psychology. By WILLIAM McDOUGALL. London:
Methuen & Co. 1923. Pp. xvi + 456. 12/- net.

BY JAMES DREVER.

PROBABLY no single living writer has exercised more influence on present-day psychology than Prof. McDougall. His *Introduction to Social Psychology* is a veritable landmark in the history of psychological development in the early years of the present century. It may with truth be asserted that the influence of that work has been felt in every branch of modern psychology. Its findings are the basis of the psychology upon which writers in many diverse fields rely when they have to touch upon psychological problems. Under these circumstances the publication of Prof. McDougall's new and important book could hardly fail to be regarded as an event of more than usual interest and significance. No psychologist of the time is surer of a large audience and a respectful hearing than is Prof. McDougall.

An Outline of Psychology is perhaps a too modest title for the work. It is more than an 'Outline' in the ordinary sense, though possibly scarcely sufficiently detailed to be called a 'Text-book,' or sufficiently comprehensive to be entitled 'Principles.' In any case, its 450 pages give much more psychology, and treat of many much more difficult problems, than we should expect the beginner in psychology to master all at once. The present writer at least believes that there is in the book material for a two years' course of psychology, and perhaps something over. But whether the book is looked on as a text for the beginner or a guide for the advanced student, all teachers of psychology must regard it as the kind of work which meets a felt need in psychology at the present time, and must accordingly feel deeply grateful to Prof. McDougall for having written it.

At the same time one cannot help saying that the reading of the book gives rise in the mind of disciples of the McDougall of *Social Psychology* to a curious mixture of feelings,-a mixture of which the chief components are gratitude to Prof. McDougall for much that he has given us, and some little disappointment at the failure to find certain things we had expected. Some of these things we may have had no right to expect, but there the expectation was all the same. Another volume is promised to treat of abnormal psychology, which will include a discussion of Freudian and kindred developments. That was one thing one might reasonably have expected to find in the present volume. Freudian psychology has significance for normal as well as abnormal psychology, and whether we follow Freud or McDougall, we can hardly ignore the findings of the other. A dismissal of the Freudian exaggeration of 'sexuality'—if it is an exaggeration—is scarcely sufficient notice of the Freudian development even in an 'outline' of normal psychology, in view of the important aspects of Freudian psychology which are quite independent of the 'sexuality' findings. At the same time we can hardly refuse an author the privilege of selecting and arranging his own subject-matter.

There is another aspect of the book which may also give rise to some little disappointment. The McDougall of the *Social Psychology* stood forward as a leader of that moderate behaviourism which has many adherents in this country, and not a few across the Atlantic. It almost looks as if that moderate behaviourism was about to lose a leader. One can sympathise with the reaction against the extreme behaviourism represented by some American writers, but such reaction must not blind us to any merits they may have. Psychology is neither philosophy nor religion, but a positive science. The facts of the universe constitute its sole court of appeal. These facts include the purposiveness of all animal behaviour, as Prof. McDougall rightly contends. Moreover, as he also points out, psychology is bound to recognise this purposive character, and must therefore resist the attempt to crush its facts into the moulds made for and by the physical sciences. The extreme behaviourist has not grasped the real nature of 'behaviour.' With these facts in view the present writer feels that the definition of psychology as the 'science of the human mind' marks a regression to an older, and scientifically much less valuable and less defensible, point of view for the science. Sherrington has pointed out that the action in a reflex really starts with the reception of the stimulus. If we carry this idea over to the complete behaviour of the living organism it is clear that all experience may be regarded as a phase of animal behaviour. The rights of the psychological view-point seem sufficiently safeguarded when we remember that a science is defined by its method of interpretation no less than its subject-matter, and that the characteristic method of interpretation adopted by psychology since it began to exist as a science has been in terms of the inner life. Psychology studies behaviour, but interprets in terms of life and mind.

Prof. McDougall's name is so closely associated with a particular theory of emotion and its relation to instinct, that one naturally turns first of all to that part of his book in which these topics are discussed. The result is that one comes upon several highly interesting points right away.

McDougall has modified the views originally presented in his *Social Psychology* in several respects, some of them by no means unimportant. First of all he discusses the relation of 'appetite' to 'instinct' on an entirely new basis. Rejecting the writer's suggestion that we should distinguish between 'appetitive' and 'reactive' tendency, which he erroneously understands as an attempt to distinguish between 'appetite' and 'instinct,' he holds that 'appetite' is a factor entering to a varying extent into instinctive behaviour but not separable from it. He seems to regard it as dependent on 'bodily metabolism,' and especially on endocrine secretion. His whole treatment of 'appetite,' however, is rather indefinite, and he nowhere deals specifically with the distinction that may be said to have been current in psychology since Plato distinguished between *ἐπιθυμία* and *θυμός* (*Rep.* Bk IV, 437 *et seq.*).

In the second place we find some modification of his original definition of 'an instinct.' As before, he regards 'an instinct' as primarily a fact of mental structure or a 'disposition.' But he points out that it is a complex disposition in which we can distinguish at least two parts:

(1) A part rendering possible the perception of the specific object in connexion with which instinctive activity takes place.

(2) A part determining the outflow of energy into the bodily organs concerned in instinctive activity. McDougall has previously used the expression 'cognitive disposition' and he repeats it several times in the *Outline*. In his Introduction he protests against the practice of separating out aspects of a total experience like 'percept,' 'idea,' and the like, and treating these as if they were entities in place of abstractions. Is he not falling into this same error, in connexion with dispositions, against which he cautions us in the case of experiences? Is there such an entity as a 'cognitive disposition,' as a separate or separable element of mental structure?

After raising the question whether we must recognise in the instinct a third part, an impulse, as a spring of energy, or driving force, and leaving the question undetermined, McDougall goes on to define 'an instinct' in the following terms:

> an innate disposition which determines the organism to perceive (to pay attention to) any object of a certain class and to experience in its presence a certain emotional excitement and an impulse to action, which find expression in a specific mode of behaviour in relation to that object.

It will be noted that 'psycho-physical' is omitted from the definition of his *Social Psychology*, but this omission is evidently of no particular importance. There is, however, another modification which can scarcely be regarded in the same light. Previously McDougall had carefully separated the emotional excitement from the action or impulse to action. Now he takes emotion and impulse together as both expressing themselves in the specific mode of behaviour. In criticising his *Social Psychology* the writer pointed out that McDougall's own discussion of instinct and emotion there failed to preserve the separation between 'emotion' and 'impulse' implied in his definition. The new definition puts that right. When McDougall comes to discuss emotions later in the book he makes a further concession to views that the writer has previously expressed, in virtually acknowledging that 'emotion' should not be used in the sense of a simple affective element, but that it always implies a complex experience, sensational, affective, and impulsive. He also shows a much more sympathetic attitude towards the James-Lange theory.

McDougall's list of human instincts has been somewhat amplified. He now enumerates 14 in addition to the "minor instincts of scratching, sneezing, coughing, urination, and defecation." As now named these instincts are: *instinct of escape, instinct of combat, repulsion, parental, appeal, pairing, curiosity, submission, assertion, social* or *gregarious, food-seeking, acquisition, construction, laughter*. Two of the additions will arouse some interest, and may possibly provoke some criticism. These are 'appeal,' the accompanying emotional quality being distress or a feeling of helplessness, and 'laughter,' the accompanying emotional quality being amusement, or jollity, carelessness, relaxation. For our part we are only concerned to criticise the last. If laughter, why not weeping? Perhaps, however, McDougall means to include that under the instinct of appeal. In that case our criticism of his 'laughter' will apply to his 'appeal' also.

The appearance of 'laughter' as an instinct is more than a little surprising in the light of the treatment of the instincts and emotions with which we are familiar in the *Social Psychology*. It is accompanied by a very interesting discussion of the theory of laughter, which one would hate to lose, and which is characterised by much that is true and much that exhibits once more McDougall's characteristically fine insight into the emotional life of the human being. But surely he is mistaken when he classifies 'laughter' as an instinct. The key with which to unlock the secrets of laughter is hardly likely to be found if we begin our search in the highly complex situation represented by the laughter of the adult.

Freud has made the same kind of mistake. If McDougall had considered the laughter of the child he could hardly have failed to see that it is the expression of any and every type and form of childish joy. Whenever the child finds the gratification of his wishes or tendencies come easily and quickly the laugh of joy is the outcome. Think of children at play. On the other hand the checking of any tendency or wish may have weeping as its outcome.

In the third Appendix to the second edition of the writer's *Instinct in Man* an attempt is made to develop a theory of the affective or emotional bipolarity of the great instincts. This theory was developed under the direct influence of McDougall's teaching concerning 'joy' and 'sorrow' in his *Social Psychology*. That teaching the writer accepted and he still accepts. It seemed, and seems, a highly important and valuable contribution to our understanding of the emotional life, and it has the closest bearing on the phenomena of laughter and tears. As McDougall rightly says, the explanation of the laughter produced by tickling is a crucial test of a theory of laughter. It is *one* such test. But another equally crucial test is the rapid alternation from laughter to tears which we find in certain types of mental disturbance, normal as well as abnormal. One has great difficulty in seeing how McDougall's theory could be applied in such cases—difficulty at least as great as the difficulty of applying the 'joy' theory to the case of tickling. While on this topic it is perhaps worth noting that McDougall's somewhat summary rejection of the writer's views regarding the nature of the emotional phase of affective experience suggests that these views have not been clearly understood, particularly in the modified form in which they are expressed in the above-mentioned Appendix.

Two other points in connexion with the treatment of the affective aspect of experience call for some notice. McDougall rejects the view that action or conation is ever determined by pleasure or pain, as, so to speak, prime movers. He would substitute what he calls the 'hormic theory' for psychological hedonism, and we admit the cogency of his argument, and are prepared to accept his conclusions in the main. It must be admitted that the regulating, not the motivating, function, is the essential biological function of pleasure-unpleasure. Nevertheless with respect to human behaviour McDougall's position appears to be too extreme. His analysis of the motives of the worthy Glasgow citizen, who took his steamer trip in order to get a good appetite so as to enjoy the excellent food provided on board, somewhat savours of special pleading. In the writer's opinion all the human appetites involve a pleasure-unpleasure motivation, but that view McDougall does not accept. Yet it seems impossible to understand how the pursuit of pleasure—even sense pleasure—does come to be an end, which apparently he would admit, unless in some sort pleasure-unpleasure was potentially a motive from the start. One cannot satisfactorily meet the teaching of Freudians regarding the sway of the 'pleasure principle' by simply substituting a hypothetical mind for the concrete human being, for that is what it seems to amount to.

The other point is with regard to 'interest.' A very excellent suggestion is made in Baldwin's *Dictionary*, that, when we use the word 'interest,' we should specify by saying 'disposition-interest' or 'experience-interest,' as the case may be. In this case McDougall is speaking of 'experience-interest.' At least that is the impression we get. And he holds that it is essentially conative. Surely there is some confusion here. Interest seems to the writer to be more characteristically *affective* than any other phase or aspect of our experience. It is the feeling of 'worth-while.' Moreover, it appears also to be the key to psychological *meaning*. But that is another story, which we do not intend to enter on at present.

It would be a great pity if these criticisms conveyed the impression that Prof. McDougall's book suffered from several grave defects. That is very far from the impression which the writer wishes to convey. The points touched upon are all important, if not fundamental, points in relation to any psychological theory of feeling and emotion. They are also highly controversial points. In every case McDougall has given a definite lead, and whether we are prepared to accept his conclusions or not, that in itself is a valuable service to psychology. In many other respects, too, the book gives the same clear lead—with respect to perceptual thinking, the nature of meaning, the growth of the structure of mind, reason and belief, and the like.

There is certain to be much controversy in various fields of psychology for many years to come, and in such controversy McDougall's influence and teaching is bound to play an important part. Some who agree with his main line of thought may decline to accept the letter of his teaching at every point, and may at this point or at that raise objections and put forward other views. But on the fundamental questions at issue in modern psychology a great and increasing mass of psychological opinion looks to him for guidance and inspiration.

PUBLICATIONS RECENTLY RECEIVED

The Psychology of Reasoning. By EUGENIO RIGNANO. Translation by WINIFRED A. HOLL. London: Kegan Paul, Trench, Trübner & Co. (International Library of Psychology, Philosophy, and Scientific Method.) 14*s.* net.

The publication of Rignano's *Psychologie du Raisonnement* some two years ago in French and Italian roused much less interest in this country than the importance of the book merited. With the publication of an English translation there is some prospect of this being rectified. Professor Rignano is the editor of *Scientia*, and some portions of the book before us have already appeared in that journal. The author's name is associated with an interesting theory of the instincts, developed from the standpoint of Richard Semon's *Mneme.* In the present work he has extended his theory to cover the higher mental operations which we designate 'reasoning'—hence the title—but the book is more than a psychology of reasoning in the narrow sense.

Rignano starts by assuming that all activity on the part of an organism springs from its fundamental tendency to maintain or to restore its 'stationary' physiological state. Interesting analogies with Freud's 'pleasure principle' suggest themselves here. All needs of the organism, and hence all affective tendencies or instincts, arise from this fundamental tendency, but owe their specific form to specific 'mnemic accumulations' due to what has happened to the organism in the course of its racial and individual development. These views are already familiar to the English reader in Rignano's *Essays in Scientific Synthesis.* From his theory of the affective tendencies the author passes on to 'attention.' The essence of attention he regards as conflict arising from the coexistence of two affective states. Hence we have an affective tendency held 'in suspense.'

At this point Rignano comes upon his main topic, 'reasoning.' Reasoning he takes to be simply performing a series of operations or experiments mentally, in imagination, in place of actually. The whole discussion is based on an affective theory of 'meaning,' which is itself of high interest and deserves the most careful consideration. Two chapters are devoted to the 'evolution of reasoning,' the first dealing with the transition from concrete reasoning to abstract, the second with the transition from 'intuition' to deduction. The author then goes on to discuss in turn the two types of reasoning we find in the higher forms—what he calls 'constructive' reasoning and 'intentional' reasoning. The two are distinguished from one another by the fact that 'constructive' reasoning aims only at the discovery of truth, whereas 'intentional' reasoning aims at the demonstration of the accuracy of a conclusion at which we have already arrived, and in which therefore we have a personal interest. Mathematical reasoning may be taken as an illustration of the first, metaphysical reasoning of the second. The discussion of mathematical reasoning is clear, cogent, and psychologically of high value. On the other hand, the two chapters devoted to 'Dialectic Reasoning' and 'Metaphysical Reasoning' respectively are written in a rather controversial spirit throughout, and, interesting though they are, this somewhat detracts from their value as scientific psychology.

Next we have a discussion of the pathology of reasoning in three chapters. The first of these chapters is devoted to 'Dreams.' Rignano wholly rejects the Freudian view of dreams. He maintains that all the characteristic features of the dream arise from the fact that in sleep, and therefore in dreams, the affective life is suspended. Non-affectivity is the key to the psychology of the dream. This involves the absence of reasoning and the functioning of 'the mechanism of evocation pure and simple.' Dreams afford the best evidence at our disposal against the associationist account of reasoning. The following two chapters are devoted to various forms of insanity interpreted in terms of disorders of the affectivities. In the last chapter on 'Conscious and Unconscious Reasoning' another very interesting section is found, where the author deals with the nature of 'consciousness' and its relation to the 'unconscious.' The theory sketched by Rignano is in many respects closely akin to that developed by Lloyd Morgan in his Presidential Address to the British Association. He finds the distinctive mark of the 'conscious' in recognition, and bases that in turn on 'affective fusion.'

The admirably clear exposition, which is characteristic of Professor Rignano's book from start to finish, makes it a valuable addition to our psychological literature. It is all the more to be welcomed because it presents a point of view which is very inadequately represented in English psychological literature. However much we may disagree with Rignano's conclusions, and however disinclined we may be to accept his point of view, neither can be ignored. The whole work is specially relevant to the problems raised by the recent publication of Spearman's important work on the *Nature of Intelligence.*

Statistical Method. By TRUMAN L. KELLEY, PH.D., Professor of Education in Stanford University. New York: The Macmillan Co. 1923. Pp. 390. 18*s*. net.

This volume fills a distinct gap in the available handbooks of statistical method, as hitherto there has been no one volume in which the student could find accounts of many of the more advanced mathematical methods, for which in many instances he had to refer to the original papers. The student will find most of such methods in this volume, though in some cases rather summarily treated. It is, on the whole, rather a work for the student who is already well grounded than for the beginner (although it nominally begins at the beginning) and also (inevitably) for the student who is a fair mathematician. Methods that find most of their applications in psychological work receive special attention.

Studies in Mental Deviations. By Professor S. D. PORTEUS, The Training School at Vineland, N.J., Department of Research. 1922. Pp. xi + 276.

For some years past Professor Porteus has directed the research work at the Vineland Training School. His plan of work, to use his own words, included 'not only the application of mental tests, but the study of the individual from the standpoints of his physical development, his industrial aptitudes and skill, his educational requirements, and the traits of his personality that also affect his capacity for self-management and self-support in the community of his fellows.' This short volume presents the results of work well conceived, carefully planned, and skilfully executed. It is throughout lucid and interesting, and is undoubtedly a real contribution to the difficult study of the training of the feeble-minded. After an account of his anthropometric examinations, the author devotes a section to the consideration of his well-known maze tests. He then discusses in a helpful way 'personality traits,' and case histories, and proceeds to more special studies of rating scales of the mentally deficient for various purposes in connexion with education and industry. There is nothing sensational about the book, but as a workmanlike survey of a large amount of painstaking investigation it is well worth the attention of all psychologists interested in the aptitudes of children.

A Psychological Retrospect of the Great War. By W. N. MAXWELL, M.A., with a foreword by Professor JOHN LAIRD. London: George Allen & Unwin, Ltd. 1923. Pp. 191. Price 6*s*. net.

Mr Maxwell first presents a brief account of the fundamental notions of certain modern developments in psychology, dealing particularly with the mode of organisation of the personality. He then attempts an analysis of 'the war impulse,' and an account of the conditions and variety of expression of 'danger instincts at the front.'

A very interesting chapter on the function of sentiment in war is followed by a study of courage. Then comes the inevitable chapter on 'the unconscious mind,' a chapter not quite as good, in fact, as most of the others. The final study of 'the influence of the group life upon the individual in the Army' is extremely interesting so far as it goes.

The book as a whole is good and was worth doing. It would have been better still if the author had written more immediately out of his own experiences, and less at the general bidding of the accepted books. It cannot claim great originality, but it should help to suggest to a large number of readers the psychological bearing and mechanism of the greatest experiences of their life.

Readings in General Psychology: Collected by Professor E. S. ROBINSON and FLORENCE RICHARDSON ROBINSON. Chicago: University of Chicago Press. 1923. Pp. xvi + 674.

This book is intended for the student of psychology who, although he is beginning his studies, wishes to make the acquaintance of the views of varied authors on points which interest him. Extracts, for the most part rather short ones, from a very large number of writers have been gathered together and reprinted, sometimes with slight alterations. The matters dealt with cover almost the whole ground of psychological investigation. The main difficulties are the great variety of views expressed, and the fact that hardly any author can be given sufficient space to develop his view properly. Yet if his readings in this book are adequately checked by some satisfactory general course, and if he is willing to supplement what he will find here by more extensive study of the works of the authors represented, the student may well profit from these 'readings in general psychology.' Without checks of this sort the book can but do harm, for although the extracts are for the most part admirably selected, the reader can get from them no more than a smattering of knowledge. It is, however, only fair to state that the volume is put forth definitely as a supplement to a full course.

NOTES ON RECENT PERIODICALS

Psychologische Forschung.

Bd. II, Hefte 1 & 2, 1922.

Versuche über das stereoskopische Sehen. (Ernst Lau.)

A short proof by means of experiments causing optical delusions that stability of identical retinal points stimulated is not the fundamental factor of stereoscopic vision.

Beitrage zur Psychologie der Gestalt VII. Experimentelle Untersuchungen uber das Entstehen und Vergehen von Gestalten. (Erich Lindemann.)

The seventh of a series of papers edited by K. Koffka and proceeding from the Psychological Laboratory of Giessen University. In it is given a detailed account of experiments on the appearance and disappearance of form, and of conclusions drawn therefrom. These seem to confirm the structure theories of Wertheimer and Köhler. They deal with the visual sensations of movement, expansion and contraction, experienced on presentation of figures under temporary or permanent illumination; these movements are shown to be connected with and in the service of the perception of structure, and their variations are determined by the tendency towards good and simple form.

Die psychologische Bedeutung magischer Bräuche. (T. W. Danzel.)

In this short paper Dr Danzel argues that magical customs, rituals, etc., were in their origin essentially an effort on the part of primitive man to alter his subjective attitude rather than the objective world.

Bemerkungen zu einer 'Grundfrage der Akustik und Tonpsychologie.' (E. M. von Hornbostel.)

A severe criticism of a paper by Lachmund (see this *Journal*, vol. XIV, I), dealing with experiments in connection with Myers' apparatus for localisation of sound.

Das Problem der Willensmessung und das Grundgesetz der Assoziation. II. (Karl Lewin.)

This second contribution to the examination of the above problem begins with a description of experiments by which mistakes in certain required activities (rhyming, reproduction, transposition, etc.) concerning series of syllables were brought about more or less at the experimenter's will. The following are the most important conclusions drawn:

(1) The fundamental law of association in its customary formulation is incorrect. An association (or habit) represents no motor power which on the appearance of one experience forces the reproduction of the other experience usually following it.

(2) Of far greater importance are the various types of activity called into being, and the attitude of readiness for any such activity, whether in connexion with an 'instinctive' act, or a 'willed' act.

Such a state of readiness for activity is the essential condition of a tendency to reproduction.

(*a*) A readiness for a particular activity *may* be brought about or ended by a particular act of will, but not in all circumstances: states of readiness for other activities independent of any act of will may come into play, and these are of great qualitative and quantitative importance.

(*b*) The individual's will cannot be measured by means of an 'associative equivalent' in Ach's sense. Number of repetitions plays only an indirect rôle, including that of mechanisation of the process, and not the *strength* of intention but the *type* of activity-preparedness it brings about determines the result.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETING.

December 15, 1923.	A Note on the Psychological Significance of the Psycho-galvanic Reaction, by H. M. Wells.
	Visual Perception Tests, by J. Kay.

SECTIONAL MEETINGS.

Aesthetics Section.

October 26, 1923.	Memory Drawing, by John D. Batten.

Education Section.

October 8, 1923.	Comparison of Certain Group Intelligence Tests, by J. H. Wilson.
November 19, 1923.	The Philosophy of Signor Gentile, by T. P. Nunn.
December 10, 1923.	Vocational Tests in Music, by Ll. Wynn Jones.

Medical Section.

October 24, 1923.	The Castration Complex in Women, by Dr Bousfield
November 28, 1923.	The Sting of Death, by M. D. Eder.
December 19, 1923.	The Relationships of the Neuroses, by Dr E. Miller.

JOINT MEETING.

General Meeting and Aesthetics Section.

November 17, 1923.	What Artists Want from Psycho-Analysis, by Roger Fry.

VOLUME XIV APRIL, 1924 PART 4

THE PRINCIPLES OF VOCATIONAL GUIDANCE (I)[1]

BY OTTO LIPMANN.

DIE Berufszuordnung kann auf zwei Wegen vor sich gehen: Entweder man sucht, eine bestimmte Person *X* in den für sie passendsten Beruf zu bringen (*Berufsauslese*, Berufsberatung, vocational guidance),

Person *X* ↗ Beruf *A*
„ *B*
„ *C*
u.s.w.

oder man sucht für einen bestimmten Beruf *A* die für ihn passendsten Personen (*Personenauslese*, Konkurrenzauslese, vocational selection)

Beruf *A* ↗ Person *X*
„ *Y*
„ *Z*
u.s.w.

Die Berufsauslese geht somit von der *Personenkunde*, der Kenntnis der Eigenschaften einer Person aus

Person *X* ↗ ↘ Eigenschaft *m*
„ *n*
„ *o*
„ *p*
„ *q*
u.s.w.

und setzt voraus, dass von jeder psychischen Eigenschaft bekannt ist, welcher Grad dieser Eigenschaft für den einzelnen Beruf erforderlich ist, für welche Berufe der Besitz oder der Nichtbesitz dieses oder jenes Grades einer Eigenschaft ihren Träger geeignet oder nicht geeignet erscheinen lässt (*Eigenschaftskunde*).

Eigenschaft *m* ↗ + Beruf *A*
„ *B*
↘ − „ *C*
u.s.w.

Die *Personenauslese* dagegen geht von der *Berufskunde* aus und setzt voraus, dass für den in Frage stehenden Beruf bekannt ist, welche Eigenschaften und welche Grade dieser Eigenschaften er erfordert, sowie welche Eigenschaften für eine gute oder wenigstens ausreichende Berufstätigkeit schädlich oder hindernd sind.

Beruf *A* ↗ + Eigenschaft *m*
„ *n*
↘ − „ *o*
„ *p*
u.s.w.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July, 1923.

Diese Schemata sind jedoch, vom Gesichtspunkte der Gestaltpsychologie ausgesehen, nicht haltbar. Denn weder darf eine Persönlichkeit als die Summe ihrer, als Bausteine aufgefasster Eigenschaften, noch darf ein Beruf als die Summe einzelner Betätigungsweisen aufgefasst werden. [Schon die Bezeichnung einer 'Eigenschaft' als solcher ist eine Konzession an die analytische Psychologie, zu der wir aber gezwungen sind, weil die Gestaltpsychologie entsprechende Termini noch nicht geprägt hat und wohl ihrem Sinne nach auch garnicht prägen kann.] Daher gibt sowohl jede Personenkunde wie jede Berufskunde ein nur unvollständiges und schiefes Bild von dem zu beschreibendem Sachverhalt, und daher kann eine darauf begründete Berufszuordnung nur in Ausnahmefällen restlos befriedigend ausfallen. Ein theoretisch restlos befriedigendes Verfahren wäre nur dies, dass man die Personen und die Berufe als *Ganze* einander probeweise zuordnet und zusieht, ob und inwieweit die betrachtete Persönlichkeit als Ganzes und der betrachtete Beruf oder eine ihm analoge Probetätigkeit als Ganzes zueinander passen. Dieser Weg ist jedoch praktisch nicht gangbar und führt da, wo er betreten wird, zu anderweitigen praktischen Schädigungen. Wir sind daher darauf angewiesen, den Bedenken, die wir gegen die Verwendung der analytischen Personenkunde und Berufskunde haben, auf andere Weise Rechnung zu tragen und die Fehler, die mit der Verwendung dieser Methoden verbunden sind, auf das mögliche Mindestmass einzuschränken.

Dieser Fehler führt um so mehr in die Irre, je isolierter eine Eigenschaft betrachtet wird, je mehr von ihrem Zusammensein und Zusammenspiel mit anderen Eigenschaften und davon, dass sie eben eine Eigenschaft einer Person ist, abstrahiert wird, und je elementarer und spezialisierter die betrachtete Eigenschaft ist. Wir können noch eher etwa die Fähigkeit eines Menschen, sich einer Aufgabe mit Eifer und Sorgfalt hinzugeben, oder seine Eigenschaft, die Aufmerksamkeit schweifen zu lassen, als charakteristische Züge seiner Persönlichkeit betrachten als etwa den Grad seines Augenmasses oder seiner Handgeschicklichkeit.

Wir können ferner die These aufstellen, dass die wahrhaft charakteristischen oder 'centralen' Züge einer Persönlichkeit sich am besten und deutlichsten in ihrem *spontanen* Verhalten enthüllen, und dass umgekehrt eine Eigenschaft um so charakteristischer für eine Persönlichkeit ist, je deutlicher sie sich in spontanem Verhalten kundgibt. Dies ist ein Grund, warum wir der Methode der *Beobachtung* des spontanen Verhaltens vor jeder anderen den Vorzug geben, und weshalb wir—abgesehen von noch zu erörternden sozial-politischen Erwägungen—die vorwiegend auf die Beobachtung gegründete Berufsauslese für den richtigeren Weg der

Berufszuordnung halten als die Personenauslese. Denn während es das höchste Ziel der Berufszuordnung sein muss, für eine Person denjenigen Beruf zu finden, in dem sie sich in einer ihr möglichst adäquaten und *natürlichen* Weise betätigen kann, geht die vorwiegend auf experimentelle Feststellungen gegründete Personenauslese davon aus, wie der Prüfling sich unter experimentellen, d. h. *künstlichen* Bedingungen verhält, in denen er nicht seine spontane, sondern nur seine reaktive Verhaltungsweise äussern kann. Neben allen sonstigen Eigenschaften, deren Prüfung Gegenstand eines Prüfungsexperimentes ist, wird immer auch unbewusst und unbeabsichtigt eine weitere Eigenschaft des Prüflings, seine Reaktionsfähigkeit, seine Fähigkeit, sich in künstliche Verhaltensbedingungen hineinzuversetzen, mitgeprüft, und es kann keinem Zweifel unterliegen, dass diese Eigenschaft die Ergebnisse sowohl in der einen wie in der anderen Richtung stark verfälschen kann. Man könnte nun zu Gunsten des Prüfungsexperimentes das Argument anführen, dass eben auch diese Reaktionsfähigkeit eine berufswichtige Eigenschaft sei, deren Feststellung im Interesse einer guten Berufszuordnung durchaus erwünscht sei. Dieser Einwand jedoch verwechselt die Reaktionsfähigkeit mit der Reaktivität: wir prüfen im Prüfungsexperiment nicht die in der Tat berufswichtige Reaktivität, sondern eben nur die Reaktionsfähigkeit; wir können nur feststellen, in welchem Grade der Prüfling sich den künstlichen Bedingungen des Prüfungsexperimentes anpassen *kann*, nicht aber, in welchem Grade er sich überhaupt ihm unnatürlichen Bedingungen anzupassen *pflegt*, wie rasch z. B. sein Interesse und sein Eifer erlahmt, wenn die künstlich hervorgerufenen Bedingungen für ihn nicht mehr ganz neue sind, u.s.w.

Das Prüfungsverfahren kann die Arbeitsbedingungen *verschlechtern* und zwar für solche Prüflinge, die eine geringe Reaktionsfähigkeit besitzen, die durch die ihnen neuen Bedingungen verwirrt werden u. dgl.; es kann die Arbeitsbedingungen *verbessern* und stellt gewissermassen *optimale* Arbeitsbedingungen für solche Prüflinge dar, die rasch und gut sich auf neue Bedingungen einstellen, die durch die Neuheit der Bedingungen interessiert werden, oder die durch den Zweck der Prüfung zu höchster Anspannung ihrer Leistungsfähigkeit veranlasst werden—wobei aber immer damit zu rechnen ist, dass Interesse und Arbeitswille unter normalen Arbeitsbedingungen ganz andere sein können. Je weniger 'lebensfern' die Methodik des Prüfungsexperimentes erscheint, je weniger kompliziert und für den Prüfling unverständlich die Apparatur ist, desto eher wird ein Schluss von der Reaktionsfähigkeit auf die allgemeine Reaktivität möglich sein.

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Jedenfalls ist auch für die Feststellung der Reaktivität die Methode der Beobachtung die geeignetere; denn die Beobachtung kann und soll sich auch auf das spontane Verhalten einer Person in solchen Situationen erstrecken, die für sie neu und unnatürlich sind. Es handelt sich dabei sowohl um solche Situationen, in denen wie im Experiment, eine bestimmte Verhaltungsweise vorgeschrieben ist—z. B. durch die Ethik oder durch die Schulordnung od. dgl.—wie auch um solche Situationen, in denen es kein 'richtiges' und kein 'falsches' Verhalten gibt. Zu den wichtigen Gelegenheiten für die Beobachtung der Reaktivität gehört auch das Prüfungsexperiment—abgesehen davon, dass, wie wir gesehen haben, die Reaktionsfähigkeit in das Ergebnis des experimentellen Befundes mit eingeht und dieses im gewissen Sinne verfälscht.

Wir halten es also für wichtig, die Technik von Prüfungsexperimenten immer so zu gestalten, dass diese nicht nur auf quantitative Ergebnisse gerichtet sind, sondern ausserdem auch Gelegenheit zur Beobachtung des qualitativen Verhaltens des Prüflings gewähren. Dies führt zu drei Forderungen:

1. Die Einzelprüfung ist der Massenprüfung vorzuziehen. Die Gruppenprüfung empfiehlt sich unter Umständen als ein Mittel, um die Reaktion des Prüflings auf eine Gruppe von Mitarbeitern zu beobachten.

2. Ein psychologisches Experiment kann immer nur von einem geschulten Psychologen veranstaltet und voll ausgenützt werden, selbst wenn die äussere Technik des Experimentes absolut festgelegt ist.

3. Der Versuchsleiter ist von allen, die quantitativen, objektiven oder sozusagen 'direkten' Ergebnisse betreffenden Registrierarbeiten zu entlasten, sodass er seine volle Aufmerksamkeit der Beobachtung der qualitativen Verhaltungsweise des Prüflings zuwenden kann. Dies ist zu erreichen, entweder indem der Prüfling seine Reaktionen selbst verzeichnet, oder durch Verwendung selbstregistrierender Apparate oder durch die Verwendung eines Protokollführers. Jedenfalls sollen immer neben 'objektiven' auch 'subjektive' Protokolle geführt werden, und der den Versuch leitende Psychologe soll nur mit den subjektiven Aufzeichnungen befasst sein.

Wir haben gegen das Prüfungsexperiment, was seine quantitativen Ergebnisse anbetrifft, hauptsächlich zwei Bedenken geäussert, nämlich erstens, dass es isolierte 'Eigenschaften' des Prüflings festzustellen unternimmt, und zweitens, dass es unnatürliche Bedingungen schafft. Diesen beiden Einwänden wird die sog. 'Arbeitsprobe' wenigstens bis zu einem gewissen Grade gerecht, jedenfalls in höherem Grade als die analytische 'Funktionsprüfung,' auf die ich noch zu sprechen komme. Die Arbeitsprobe stellt erstens den Prüfling vor Aufgaben, die berufliche Arbeiten in ihrer ganzen Komplexität nachahmen, und sie lässt zweitens diese Arbeit so lange ausführen, bzw. so oft wiederholen, bis sie von ihrer Künstlichkeit verlieren und wenigstens als quasi-natürliche Verrichtungen angesehen werden können. Zweifellos stellt somit die Arbeitsprobe einen Fortschritt gegenüber der Funktionsprüfung dar, und demgegenüber muss ihr Mangel, dass sie den Prüfling sehr viel länger —bisweilen während mehrerer Tage—beansprucht und nicht zu so raschen Ergebnissen führt wie die Funktionsprüfung, wohl in Kauf genommen werden.

Es war ein Ausgangspunkt unserer Betrachtung, dass sowohl die Persönlichkeit eines Berufsuchenden wie ein Beruf nicht als Summe nach Betätigung verlangender bzw. geforderter Eigenschaften sondern als Gestalt-Ganze aufgefasst werden müssen. Nun gibt es aber zweifellos eine ganze Anzahl beruflicher Tätigkeiten, in die eine Person als Ganzes sich garnicht einordnen lässt. Ebenso, wie es auf der einen Seite Berufe gibt, welche eine ganze Persönlichkeit erfordern, die völlig befriedigend nur von demjenigen ausgeübt werden können, der mit ganzer Seele dabei ist, so gibt es auf der anderen Seite Berufsarbeiten, welche die Seele des Arbeitenden garnicht berühren und nur ganz periphere Betätigungen verlangen. In diese Gruppe gehören fast alle sog. 'ungelernten' Arbeiten —wie Packen, Ordnen, Sortieren, Einpassen, u. dgl.—bei denen meistens nur eine oder höchstens einige sehr wenige fest umschriebene Tätigkeiten, die meist mehr oder weniger rasch mechanisiert werden, immer wieder zu wiederholen sind.

[Es hat keinen Zweck, immer wieder Betrachtungen darüber anzustellen, ob die immer mehr fortschreitende, durch den fortschreitenden Prozess der Arbeitsteilung und der Mechanisierung der Arbeit bedingte Vermehrung dieser Art von Berufsarbeiten erwünscht ist oder nicht. Aber gegenüber den Stimmen, die eine derartige Entseelung und Entpersönlichung der Arbeit beklagen, muss doch auf die, ethisch vielleicht bedauerliche, aber psychologisch durchaus verständliche Tatsache hingewiesen werden, dass eine grosse Zahl von Arbeitenden, besonders von arbeitenden Frauen durchaus damit einverstanden ist, dass die Berufsarbeit ihre Seele nicht berührt, und dass sie unter der Monotonie dieser Verrichtungen durchaus nicht leiden. Eine psychologische Arbeitsrationalisierung muss nur darauf Bedacht nehmen, dass die monotonen Arbeiten auch wirklich nur periphere Anforderungen an den Arbeiter stellen und das Centrum der Persönlichkeit z. B. die Aufmerksamkeit des Arbeiters nicht beanspruchen; mit anderen Worten: dass sie leicht und vollständig mechanisierbar sind. Nur solche Arbeiten, die zugleich monoton sind und eine ständige Aufmerksamkeit des Arbeiters verlangen, weil bei Unaufmerksamkeit Gefahren für Leib und Leben des Arbeitenden oder anderer Personen oder Sachverluste eintreten können, entsprechen den Forderungen nicht, die vom Standpunkte des geistigen und körperlichen Arbeiterschutzes aus erhoben werden müssen.]

Von den einen ganzen Menschen und eine ganze Seele erfordernden '*Berufen*' bis zu den in einer einzigen monotonen Tätigkeit bestehenden '*Arbeiten*' läuft eine grosse Reihe von Übergängen, die z. T. dadurch gegeben sind, dass auch in den eigentlichen Berufen oft mehr oder weniger mechanische Hilfsarbeiten eine Rolle spielen. Je näher eine Berufstätigkeit dem Extrem der peripheren 'Arbeit' steht, je weniger sie den Einsatz der ganzen Persönlichkeit der Berufstätigen erfordert, in desto höherem Grade lässt die ihr entsprechende 'Arbeitseignung' sich mit Hilfe einer analytischen Funktionsprüfung feststellen. Wir unterscheiden also 'Berufseignung,' d. i. die Eignung für die Ausübung eines komplexen, das Ganze einer Persönlichkeit erfordernden 'Berufes,' und die 'Arbeitseignung,' die Eignung für die Ausübung einer spezialisierten, peripheren, dem Ausübenden als Verrichtung gleichgültigen 'Arbeit' (die aber als Mittel zum Zwecke z. B. des Erwerbs durchaus nicht gleichgültig zu sein braucht). Unter den Mitteln für die Feststellung von Berufseignungen steht die Beobachtung des spontanen und gegebenenfalls auch des reaktiven Verhaltens und nächstdem die Arbeitsprobe in erster Reihe; sofern die Beobachtungsmethode heute ein vollwertige Methode noch nicht ist, so kann sie doch zu einer solchen weiter entwickelt werden; als Mittel für die Feststellung von Arbeitseignungen kommt neben der Arbeitsprobe auch die analytische Funktionsprüfung in Betracht.

Die analytische Funktionsprüfung geht also im Unterschiede von der Beobachtung und der Arbeitsprobe von einer undsummenhaften Analyse einer Arbeit aus, was, wie wir gesehen haben, *hier* bis zu einem gewissen Grade zulässig ist. Doch muss auch hierbei noch einmal auf die Gefahren der psychologischen Terminologie, die 'idola fori' Bacons, hingewiesen werden, die dazu führen kann, früher vielfach dazu geführt hat und auch heute noch hier und da dazu führt, den Arbeitsvorgang als die Summe der Wirkungen, die Person des Prüflings als die Summe des Wirkens von vielleicht fünfzig, in ihrer Stärke variablen, aber ihrer Art nach fest umgrenzten psychischen Eigenschaften zu betrachten. Wenn eine Arbeitsanalyse ergeben hat, dass die Arbeit Augenmass, Handgeschicklichkeit, Gedächtnis, Aufmerksamkeit und Intelligenz erfordert, so dürfen nun nicht etwa diese Eigenschaften als festumschriebene Grössen betrachtet werden, sodass es nur noch festzustellen gilt, in welcher Stärke der Prüfling sie besitzt; sondern diese Begriffe sind noch rohe Verallgemeinerungen ganzer Klassen von Verhaltungsweisen und stehen weder ihrem Inhalte noch ihrem Umfange nach fest. Es kann sich bei der Prüfung nicht darum handeln, festzustellen, in welchem Grade der Prüfling 'Intelligenz' *überhaupt* besitzt, sondern nur darum, wieweit er über diejenige Intelligenz verfügt, die eben bei der Ausübung der in Frage stehenden Arbeit zu betätigen ist. Mit anderen Worten, eine der Testprüfung zugrundeliegende Arbeitskunde führt irre, wenn sie sich allgemein-psychologischer Fachausdrücke bedient; der Testprüfung kann vielmehr nur eine *Beschreibung* des Arbeitsvorganges zugrundegelegt werden. Es sind dann die einzelnen Teilvorgänge zu trennen und in der Testmethodik nachzuahmen oder zu analysieren. Denn was von der Arbeitskunde gilt, gilt ebenso auch von der Testmethodik: es gibt keine Methode, die es gestattet, *die* Aufmerksamkeit oder *die* Handgeschicklichkeit, *die* Intelligenz eines Menschen zu prüfen; und ebenso irreführend sind Bezeichnungen von Apparaten als 'Mnemometer,' 'Konzentrationsprüfer' u. dgl. Immer werden hierbei ganz spezifische Teilfunktionen geprüft, also erstens nicht die im Namen angedeutete Gesamtfunktion—die es garnicht gibt—und zweitens gleichzeitig eine grosse Anzahl von Nebenfunktionen, die als Bedingungen des Experiments auftreten, seine spezifische Form mitbestimmen, aber im Interesse einer bequemen Terminologie unterschlagen werden.

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Es soll nicht behauptet werden, dass nicht unter den einem psychologischen Oberbegriff untergeordneten Teilfunktionen gewisse korrelative Beziehungen vorliegen, aber solange darauf bezügliche spezielle Untersuchungen nicht angestellt sind und *hohe* Korrelationen ergeben haben, ist ein blosser Wahrscheinlichkeitsschluss z. B. aus dem Verhalten etwa in einem psychologischen Test auf das Verhalten in einer scheinbar und äusserlich ähnlichen Berufsarbeit—ein Schluss, der sich etwa bloss darauf gründet, dass in beiden Fällen 'die Aufmerksamkeit' beansprucht wird—sehr leicht trügerisch.

Es kann daher nur einer ersten Orientierung dienen, wenn Beziehungen hergestellt werden zwischen den Berufen und beruflichen Arbeiten einerseits und einer Liste psychischer Eigenschaften andererseits. Es genügt nicht, dass ich weiss, für den Beruf *A* müssen die Eigenschaften *m*, *n*, *o*, *p*, für den Beruf *B* die Eigenschaften *n*, *o*, *p*, *q* geprüft werden—ein Prüfling, der bei der Prüfung der Eigenschaft *m* versagt, scheidet damit von vornherein für den Beruf *A* aus—ein Prüfling, der den Schwerpunkt seiner Begabung in der Eigenschaft *q* hat, erscheint damit prädestiniert für den Beruf *B*, u.s.w.

Deshalb erscheint uns auch die Methode der Funktionsprüfung zwar verwendbar, wenn sie auf je einen ganz bestimmten Beruf zugeschnitten ist, also für Zwecke der Personenauslese, aber nicht für Zwecke der Berufsauslese. [In der praktischen Arbeit eines Berufsamtes gestaltet sich das Verhältnis zwischen Berufs- und Personenauslese bzw. zwischen Beobachtungs- und Testmethodik häufig folgendermassen: Auf Grund der Ergebnisse der Schulbeobachtung, des persönlichen Eindruckes des Berufsberaters, der Schulzeugnisse und ärztlicher Gutachten wird für den Berufsuchenden eine provisorische Berufsauslese vorgenommen. Bleibt diese Zuordnung dem Berufsberater irgendwie noch zweifelhaft, so wird der Berufsuchende nun auf seine Eignung für *diesen* Beruf auch experimentell untersucht.]

Das vorher über die Notwendigkeit einer Spezialisierung der Testmethoden Gesagte gilt nur dann nicht oder wenigstens nicht in dieser Schärfe, wenn es sich um *physiologische* Bestimmungen der Sehschärfe, Hörschärfe u. dgl. handelt; aber es gibt selbst da, insofern in solche vermeintlich rein physiologische Bestimmungen auch psychische Verhaltensweisen, wie Aufmerksamkeit u. dgl. mit eingehen. Daher ist es z. B. wohl auch bei Augenmassprüfungen nicht ganz gleichgültig, unter welchen speziellen Bedingungen und an welchen Objekten sie vorgenommen werden.

Da die Testprüfung von der bald mehr bald weniger berechtigten Fiktion ausgeht, dass der Arbeitsvorgang aus einzelnen Teilvorgängen zusammengesetzt sei, und dass die Arbeitseignung aus einzelnen Komponenten bestehe, so soll auch das Prüfungsergebnis dieses Bild rein und unverfälscht zum Ausdruck bringen. Es ist dem Psychologen nicht gestattet, dieses Mosaikbild wieder zu *einer* Grösse, 'die Berufstauglichkeit' genannt, zu verschmelzen, als welche Funktion der den einzelnen Eigenschaften entsprechenden unabhängigen Variabeln man diese Komplexeigenschaft 'Berufstauglichkeit' auch immer betrachten möge. Sein psychologisches Gewissen muss es dem Psychologen verbieten, die Ergebnisse seiner Testprüfungen in anderer Form zur Darstellung zu bringen, als in der Form des 'psychologischen Profils.' Den Bedürfnissen der Praxis wird damit allerdings nicht genüge geleistet; denn dem Praktiker genügt es nicht, zu erfahren, dass die Person *X* hinsichtlich der Eigenschaft *m* besser, in Bezug auf die Eigenschaft *n* aber schlechter ist als *Y*, sondern er will in *einer* Zahl zum Ausdruck gebracht sehen, ob

X oder *Y* für den Beruf, um den es sich handelt, tauglicher sei. Diese Frage kann aber der Psychologe allein nicht beantworten, und ihre Beantwortung überschreitet seine Zuständigkeit. Es muss im Wesentlichen dem Praktiker überlassen bleiben, wie er in Zusammenarbeit mit dem Psychologen aus den einzelnen von diesem ermittelten Befunden einen 'Index der Berufstauglichkeit' überhaupt oder einen Index der Tauglichkeit für einzelne Zweige des Berufes berechnen will, mit welchen Gewichten er dabei die einzelnen Eigenschaften bewerten, und in welche funktionale Abhängigkeit er die Einzeleigenschaften und die Berufstauglichkeit zu einander setzen will. Selbst die Kombination der Werte für die Qualität (Fehlerhaftigkeit) einer Arbeit und die Schnelligkeit ihrer Verrichtung zu einer kombinierten Wertziffer überschreitet die Kompetenz des Psychologen, und die 'Richtigkeit' einer solchen Kombination ist ausserdem von Fall zu Fall verschieden, da es nicht nur bei verschiedenen Arbeiten, sondern sogar hinsichtlich derselben Arbeit bei verschiedenen Betrieben bald mehr auf die Güte bald mehr auf die Schnelligkeit der Leistung ankommt.

Die Frage, wieweit eine einmalige Testprüfung überhaupt zu einwandfreien Ergebnissen über die Berufstauglichkeit führen könne, ist noch nicht geklärt und gegenwärtig Gegenstand zahlreicher Untersuchungen. Auf der einen Seite werden zu Gunsten der Testmethodik die Ergebnisse von Erhebungen angeführt, wonach die im Berufe festgestellte Berufstüchtigkeit in hohem Grade mit der experimentell gefundenen Berufstauglichkeit übereinstimme. Wenn diesem Befunde ein Rangreihenvergleich, und wenn der Rangreihe der Berufstüchtigkeit Praktikerurteile zugrundeliegen, so müssen wir einem solchen Ergebnis wie es auch lauten mag, Misstrauen entgegenbringen. Noch weniger als wir bei einem einigermassen komplexen Beruf die Berufstauglichkeit einigermassen eindeutig als Funktion der einzelnen gemessenen Eigenschaften definieren können, ist es dem Praktiker möglich, auf Grund heterogener, subjektiver Eindrücke eine Rangreihe der Berufstätigen nach dem Grade ihrer gleichfalls sehr komplexen Berufstüchtigkeit aufzustellen.

Etwas günstiger liegt diese Möglichkeit einer Korrelierung zwischen Berufstüchtigkeit und Berufstauglichkeit, wenn die Berufstüchtigkeit an objektiven Kriterien gemessen werden kann: Höhe der erzielten Akkordlöhne, Zahl der unbrauchbaren Arbeitsprodukte, Dauer der Anlern- oder der Lehrzeit bis zum Beginn produktiver Arbeit, Zahl der verschuldeten Unfälle, Material- und Kraftverbrauch u. dgl. Um für den Lehrling noch vor Beendigung seiner Lehrzeit ein Kriterium für seine Berufstüchtigkeit zu erhalten, hat man vorgeschlagen, ihn Probestücke anfertigen zu lassen und die Berufstüchtigkeit an der Qualität solcher Probearbeiten und an der Schnelligkeit ihrer Herstellung zu bemessen. Auch diese Methode führt, wie Argelander[1] nachgewiesen hat, leicht zu Trugschlüssen, da die Zeiten, die eine Gruppe von Lehrlingen im Laufe von 4 bis 5 Monaten zur Fertigstellung von je 5 Probestücken brauchten, nur sehr geringe, wenn überhaupt positive Korrelationen ergaben. Was hier von den Probestücken gilt, dass sie nämlich qualitativ *verschieden* und qualitativ verschieden *schwierig* sind, und deshalb auch verschiedene Seiten der Berufstüchtigkeit beanspruchen, gilt natürlich auch und erst recht von den eigentlichen Berufsarbeiten. Man sieht also auch hier, wie wenig '*die* Berufstüchtigkeit' durch *eine* Arbeit einheitlich erfasst werden kann, dass also noch weniger '*die* Berufstüchtigkeit' *begrifflich* einheitlich zu erfassen ist.

Für einen Vergleich zwischen Berufstauglichkeit und Berufstüchtigkeit sind also alle Methoden, die auf einem *Rangreihen*vergleich basieren, bedenklich. Vorzuziehen sind die Methoden des *Gruppen*vergleichs, wobei man entweder eine mit Hilfe psychologischer Verfahren ausgelesene Gruppe mit einer nicht ausgelesenen Gruppe oder eine Gruppe auf Grund eines psychologischen Verfahrens als tauglich Befundener mit einer Gruppe als untauglich Beurteilter in Vergleich stellen kann. Inhalt des Vergleichs können der durchschnittliche oder gesamte Material- oder Kraftverbrauch, die Menge unbrauchbarer Arbeitsprodukte, die Häufigkeit der verschuldeten Unfälle, die Unterrichtserfolge während der Lehr- oder Anlernzeit u. dgl. sein; die meisten dieser Bestimmungen sind besser als Kollektivgrössen denn als Individualwerte zu erfassen.

Eine Reihe solcher Untersuchungen über die Bewährung der verwendeten Auslesemethoden haben, wie gesagt, zu günstigen Ergebnissen geführt. Diesen günstigen Ergebnissen aber stehen nun andere, auf einem zweiten Wege gewonnene Ergebnisse gegenüber, ohne dass bisher eine Lösung dieses Widerspruches gelungen wäre. Zum Teil freilich fällt das Bild, das wir durch die Mitteilung über die Ergebnisse durchgeführter Bewährungskontrollen erhalten, dadurch zu günstig aus, dass die Oeffentlichkeit über die ungünstigen Ergebnisse solcher Kontrollen wohl sehr häufig nichts erfährt.

Der eben genannte 'zweite Weg,' der uns zu einem Urteil über die praktische Verwendbarkeit einer Prüfmethode führen kann, ist die Konstanzuntersuchung, im Gegensatze zu der eben behandelten 'Nach-untersuchung' auch 'Voruntersuchung' genannt. Für den Psychologen hätte eine solche Voruntersuchung eigentlich nicht der der 'zweite' Weg, sondern das Allererste sein müssen, bevor er eine Prüfmethode praktisch verwendet oder zur praktischen Verwendung empfiehlt,—dass er sich nämlich vergewissert, ob diese Methode bei mehrmaliger Anwendung an denselben Personen zu relativ gleichen Ergebnissen, d. h. zu denselben Rangreihen führt. Dass dies nicht geschehen ist, und dass solche Konstanzuntersuchungen erst nachhinken, ist verursacht durch die überaus rasche, dem Psychologen vielfach *zu* rasche Entwicklung der praktischen Berufspsychologie.

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[1] Annelies Argelander, "Beiträge zur Psychologie der Übung. IV. Anfangsleistung und Übungsfortschritt bei industrieller Arbeit," *Zeitschrift fur angewandte Psychologie*, XXI, 246 ff., 1923.

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Bei der praktischen Verwendung einer Prüfmethode ist vorausgesetzt, dass die geprüfte Leistung überhaupt weder durch die Übung gesteigert wird noch Dispositionsschwankungen unterliegt, oder dass die individuellen Leistungen der Prüflinge durch die Übung nicht in allzu verschiedenem Grade verändert werden. In beiden Fällen müsste also die Rangreihe der Prüflinge bei mehrmaligen Prüfungen derselben Art wenigstens annähernd dieselbe bleiben.

Bogen[1] hat für einige Tests das Verhalten gegenüber Dispositionsschwankungen untersucht, während Argelander[2] das Verhalten gegenüber der Übung zum Gegenstande ihrer sehr gründlichen Untersuchungen gemacht hat.

Bogens Ergebnisse bestätigen eine von mir folgendermassen formulierte Hypothese: Je niederer Art eine Tätigkeit ist, d. h. eine je reinere Sinnesfunktion sie ist, und je weniger höhere geistige Eigenschaften, wie Intelligenz, Phantasie, Aufmerksamkeit sie erfordert, in desto geringerem Grade unterliegt sie dispositionellen Schwankungen. Hinzuzufügen ist, dass ferner auch solche Aufgaben von Person zu Person dispositionellen Schwankungen unterliegen und daher geringe Rangreihenkonstanz ergeben, 'bei deren Lösungen Fähigkeiten, deren seelische Bezogenheit aufeinander eine recht geringe ist (z. B. mechanisches Gedächtnis und beziehendes Denken) zu etwa gleichen Teilen den Ausschlag geben,' und solche, 'deren Lösung in verschiedenen Funktionsgebieten (z. B. kombinatorische Phantasie und induktives Denken) mit gleichem Erfolge gewonnen werden kann.'

Argelander fand, dass bei quantitativen Arbeitsleistungen, d. h. bei solchen, bei denen es im Wesentlichen auf die Schnelligkeit der Leistung ankommt, hohe Anfangsleistungen gewöhnlich mit geringem, geringe Anfangsleistungen mit hohem Übungszuwachs verbunden sind, und dass hier von einer Rangreihenkonstanz keine Rede sein kann. Bezüglich der Qualität (Güte und Fehlerlosigkeit) der Arbeit gilt zwar das erste Gesetz nicht, aber ein gleichmässiger Übungszuwachs ist auch hier nicht feststellbar.

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[1] Hellmuth Bogen, "Zur Frage der Rangreihenkonstanz bei Begabungs- und Eignungsprüfungen," *Zeitschrift fur angewandte Psychologie*, xx, 153 ff., 1922.

[2] Annelies Argelander, "Beiträge zur Psychologie der Übung," *Zeitschrift für angewandte Psychologie*, xx, 1 ff., 1921, und xxi, 225 ff., 1923.

Argelander zieht aus ihren Ergebnissen den Schluss, dass bei der Auslese von Lehrlingen 'nicht ohne Weiteres eine *einmalige* Eignungsprüfung für die Einstellung in den Betrieb entscheidend sein dürfte.'

Den Widerspruch zwischen der oft behaupteten praktischen Brauchbarkeit einer Methode und ihrem Verhalten zu Übungs- und Dispositionseinflüssen zu klären, ist im psychologischen Interesse dringend erforderlich. Die Frage, welche Eigenschaften mehr und welche weniger solchen Einflüssen unterliegen, ist von höchsten praktischen und theoretisch-psychologischen Interesse. Das Berufseignungsproblem ist zugleich ein technisches und ein wissenschaftliches. Man kann sich als *Techniker* auf den Standpunkt stellen, dass man es nur mit der *praktischen* Brauchbarkeit einer Methode zu tun habe, und dass man sich um die *wissenschaftliche* Grundlegung nicht weiter zu kümmern brauche, wenn die Methode sich praktisch ausreichend bewährt habe. Wissenschaftlich—und wir haben es hier bei einem wissenschaftlichen Kongress ja nur mit der wissenschaftlichen Seite des Problems zu tun—ist diese Auffassung nicht befriedigend. Es ist denkbar, dass ein Baumeister, ohne sich um statische Berechnungen zu kümmern, eine Brücke baut, und dass diese Brücke merkwürdigerweise dennoch nicht einstürzt. Dann bleibt es ein wissenschaftliches Problem zu untersuchen, wie dieser Widerspruch zwischen Theorie und Praxis zu erklären sei, aber es ist unwissenschaftlich zu erklären, dass die statischen Berechnungen überhaupt keinen Sinn und Zweck hätten. Ich persönlich bin so wenig Techniker und so sehr Wissenschaftler, dass ich einer solchen Brücke, auch wenn sie eine Weile gehalten hat, auf die Dauer doch nicht vertrauen würde.

Beziehen diese Untersuchungen sich auf Veränderungen, welche die relative Berufstauglichkeit von Prüflingen infolge der Übung innerhalb verhältnismässig *kurzer* Zeitspannen erleidet, so sind die Änderungen, welche die *Alters*entwicklung, insbes. die Pubertät in der Berufstüchtigkeit mit sich bringt, noch garnicht Gegenstand systematischer Untersuchungen gewesen. Dieses Problem ist nicht nur von theoretischer sondern auch von praktischer Bedeutsamkeit, da die Berufsberatung der *Jugendlichen* offenbar nicht auf ihrer *gegenwärtigen* Berufseignung aufzubauen ist, mit anderen Worten: nicht nur ein *diagnostisches* sondern ein *prognos-*

tisches Problem ist. Wir kommen hier noch einmal auf früher Gesagtes zurück, nämlich auf den Unterschied zwischen komplexen oder centralen Eigenschaften oder besser Merkmalen der Persönlichkeit und peripheren, sozusagen accidentellen Eigenschaften. Wir stellen hier die Hypothese auf: Je centraler eine Eigenschaft ist, in desto höherem Grade erhält diese Eigenschaft sich auch über längere Zeiträume konstant. Ist schon dies ein wichtiger Hinweis auf die Bedeutung der centralen Eigenschaften für die Berufszuordnung, so erhöht diese Wichtigkeit sich noch, wenn sich auch eine zweite Hypothese bestätigen würde: Je centraler eine Eigenschaft ist, in desto höherem Grade ist ihr Vorhandensein mit dem Wunsche sie zu betätigen verbunden, in desto höherem Grade decken sich für Betätigungen, die diese Eigenschaften erfordern, Eignung und Neigung.

Die Bedeutung der centralen Eigenschaften für die Berufszuordnung ist damit nicht erschöpft; denn die centralen Eigenschaften können vielfach auch zur Kompensation mangelhaft entwickelter peripherer Eigenschaften, sofern diese zu einer Berufseignung erforderlich sind, durch Heranziehung von Ersatzfunktionen beitragen.

Mit dem Hinweis auf die Bedeutung der centralen Eigenschaften gegenüber den peripheren propagieren wir gleichzeitig—unter Bezugnahme auf frühere Ausführungen—die Beobachtungsmethodik gegenüber der experimentellen und die Berufsauslese gegenüber der Personenauslese. Selbstverständlich verstehen wir hier unter 'Beobachtung' nur eine Beobachtung durch solche Personen, die zur psychologischen Beobachtung befähigt und in dieser Methodik geschult sind. Man kann die Unzuverlässigkeit der Beobachtungsmethode nicht dadurch zu beweisen suchen, dass man darauf hinweist, wie wenig *Vorarbeiter* und *Meister* die Berufstüchtigkeit ihrer Lehrlinge oder einzelne ihrer Eigenschaften zu beurteilen imstande sind. Wir denken, wenn wir von 'Beobachtung' sprechen, immer in erster Linie an die psychologische Beobachtung von Schülern durch ihre Lehrer. Der Lehrer aber hat es—im Unterschiede zu dem Meister—ja berufsmässig mit psychologischen Beobachtungen zu tun. Sicherlich besitzen solche Personen, deren Beruf der Umgang mit *Menschen* ist, von vornherein eine höhere Fähigkeit zur psychologischen Beobachtung, und jedenfalls ist bisher unsere Auffassung nicht widerlegt, dass diese Beobachtung durch geeignete Mittel auf ein für unsere Zwecke genügend hohes Niveau gebracht werden kann.

Auch aus sozialpolitischen Erwägungen heraus muss die Berufsauslese gegenüber der Personenauslese in den Vordergrund gestellt werden: Es ist viel wichtiger, dass jeder Mensch einem für ihn möglichst geeigneten

Beruf zugeführt wird, als dass einer Reihe von Berufen zum Schaden anderer möglichst tüchtige Menschen zugeführt werden. Denn wenn man das Prinzip und die Methoden der Personenauslese genau betrachtet, erkennt man leicht, dass den Berufen und Betrieben, die sich der Personenauslese bedienen, auch solche Personen zugeführt werden, die für den Beruf oder den Betrieb oder die Arbeit, für die sie bestimmt werden, *zu* tüchtig sind. Die der Personenauslese dienende Berufskunde hat vielfach bereits die *Minimal*anforderungen formuliert, die an einen Bewerber hinsichtlich dieser oder jener Eigenschaft gestellt werden müssen. Aber um die *Maximal*anforderungen, d. h. um die obere Grenze, jenseits derer eine Eigenschaft in einem Berufe oder Betriebe oder in einer Arbeit keine adäquaten Betätigungsmöglichkeiten mehr findet (man kann für einen Beruf auch *zu* intelligent sein u. dgl.), hat man sich noch kaum gekümmert, eben weil hierfür nicht von Seiten der *Betriebe*, sondern nur von Seiten der Arbeit*nehmerschaft* bzw. der Sozialpolitik ein Interesse vorliegt.

Wenn wir gegenüber der Personenauslese die Berufsauslese und gegenüber der experimentellen die Beobachtungsmethodik bei der Berufszuordnung stärker betont sehen wollen, so sind wir uns darüber klar, dass die Beobachtungsmethode nicht alle für ein Berufszuordnung in Betracht kommenden Personen erfassen kann, sondern nur diejenigen, die durch den auffallenden Besitz oder Nichtbesitz gewisser centraler Eigenschaften aus der Menge der überhaupt beobachtbaren Personen irgendwie abstechen. Wir sind in der Tat der Meinung, dass es genügt, wenn für *diese* Personen eine psychologische Berufszuordnung stattfindet; in der gegenwärtigen Hochflut der berufspsychologischen Bewegung mag es allerdings ketzerisch klingen, wenn wir die Meinung aussprechen, dass es für die Mehrheit der Menschen unter psychologischen Gesichtspunkten ganz gleichgültig ist, welchem Berufe sie zugeführt werden. Die Anzahl derjenigen, die einer *psychologischen* Berufsberatung bedürfen, ist nur klein im Verhältnis zur Anzahl derjenigen, die *überhaupt* zu beraten sind. Und obwohl natürlich eine Beobachtung zeitraubender ist als ein experimentelle Prüfung, so können doch die wenigen, die einer psychologischen Berufsberatung bedürfen, durchaus einer ins Tiefe dringenden Beobachtung unterworfen werden. Es kommt bei der psychologischen Berufsberatung meist nicht auf die Zuordnung zu einem bestimmten Berufe, sondern zu einer Berufs*gruppe* an. Hierzu ist es notwendig, die psychologische Verwandtschaft der Berufe weiter zu studieren und die vorliegenden Versuche zur Aufstellung eines psychologischen Systems der Berufe weiterzuführen.

Sicherlich stellen auch viele Berufe keine psychischen Anforderungen, die nicht schlechthin von jedem Menschen ohne Weiteres erfüllt werden können. Für diese Berufe und Arbeiten ist also eine Personenauslese, die sich psychologischer Methoden bedient, überflüssig. Welche Berufe und Arbeiten es sind, die einer psychologischen Personenauslese bedürfen, ist die wichtigste Frage der psychologischen Berufskunde. Sie ist zu beantworten auf Grund von Statistiken über Berufswechsel, Unfall-Häufigkeit, Material- und Kräfteverbrauch, Zahl unbrauchbarer Arbeitsprodukte u. dgl. Selbst solche Ergebnisse aber weisen nicht eindeutig auf die Notwendigkeit einer psychologischen Personenauslese hin. Wenn ein Betrieb oder Beruf hinter anderen ähnlich gearteten hinsichtlich der eben erwähnten Merkmale zurücksteht, so wird häufig die sozialpolitisch befriedigendere, wenn auch vielleicht für den Unternehmer unbequemere Massnahme die sein, dass die Betriebseinrichtungen rationeller organisiert und damit auch einem weniger ausgelesenen Arbeitermaterial angepasst werden. Wenn in einem Betriebe nur durch höchste Anspannung der Aufmerksamkeit der Arbeiter Unfälle vermieden werden können, so ist es vorzuziehen, wenn möglich die Betriebseinrichtungen weniger gefährlich zu gestalten, als bei der Arbeitseinstellung darauf Bedacht zu nehmen, dass nur solche ausgelesen werden, die zu höchster Aufmerksamkeitsleistung imstande sind.

Obwohl solche Bedenken gegen die üblichen Methoden der Personenauslese heute auch vielfach schon von Seiten der Unternehmer und Betriebsleiter anerkannt werden, wollen sie doch die Methoden der psychologischen Tauglichkeitsprüfung nicht entbehren. Sie bedienen sich überall da, wo das Angebot an Arbeitskräften, besonders an Lehrlingen die Zahl der Freistellen übersteigt, der Ausleseprüfung als eines 'objektiven' Verfahrens, auf dessen Ergebnisse sie sich gegenüber jedem Einwande der Parteilichkeit berufen können, der von Betriebsräten oder sonstigen Betriebsangehörigen erhoben werden könnte. Da die Tauglichkeitsprüfung *diesen* Zwecke zweifellos in jedem Fall erfüllt, so stehen die Betriebsleiter sonstigen Forderungen, die von psychologischer Seite an das Verfahren gestellt werden, oft ziemlich gleichgültig gegenüber.

THE PRINCIPLES OF VOCATIONAL GUIDANCE (II)[1]

By CYRIL BURT.

The present paper deals almost exclusively with vocational guidance as distinguished from vocational selection. Dr Lipmann, upon social as well as psychological grounds, has strongly argued for the higher and prior claims of vocational guidance. Often, it is true, the task of vocational selection appears the easier; the employer is more interested in the choice of the man for the job, than of the job for the man; for this purpose, too, firms are more willing to grant facilities and to find funds; the problems of selection are more definite and circumscribed; the requirements, simpler and more precise; and the discovery of separate tests for separate occupations would seem logically to precede the application of those tests, in series, to one and the same person, whose future occupation is as yet wholly undetermined. These considerations, however, though often pressed, are not the sole considerations, nor even the soundest considerations, that may direct the psychologist in deciding his line of approach. And, in the main, I am in full agreement with Dr Lipmann's arguments.

Accordingly, it is with the principles of vocational guidance that I shall be chiefly concerned; and the problems that I shall endeavour to formulate will be, for the most part, practical problems that have been actually encountered in a recent co-operative experiment upon the possibility of choosing suitable employments for children and young persons in London. To survey the whole field in a brief contribution is not possible. I propose, therefore, to put forward for debate certain urgent questions and certain tentative solutions, not so much as statements of established results, but rather as tentative hypotheses to challenge discussion.

I. AT WHAT AGE CAN VOCATIONAL GUIDANCE BE MOST SATISFACTORILY APPLIED?

With adults additional difficulties are almost always encountered. The psychological study of their aptitudes constitutes a far harder and more complex task: and, at the same time, is often the least important factor in deciding what practical recommendations are the best. A well-paid clerk, 30 years of age, finds his work in a shipping office uncongenial and applies for vocational advice; it is found that the occupation for which he is most fitted is journalism. He is too old, however, to undertake the necessary training and apprenticeship; and the prospects of a livelihood are too precarious and remote. Another, of about the same age and also a dissatisfied clerk, with a large family, proves to have a definite engineering bent; the same objection is encountered. A third, a neurotic bachelor of 45, who has been by turns a clergyman, a soldier, and a commercial traveller, is found to have an artistic temperament with all its qualities and defects. In each of these cases, owing chiefly to the man's age, it is impossible to recommend to him the profession for which he is by nature best adapted, since it is too late in life for him to start afresh, and seek the training, the education and the experience which the indicated career would necessarily require.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July, 1923.

It would appear, then, that psychological guidance is likely to be most useful when it is given at the earliest possible age. This in turn would seem to be the age when general education is finished, and the elementary (or corresponding) school is exchanged either for industrial employment or for more specialised training. This point is roughly the mental age of 14. The average child leaves school for ever in his 14th year; brighter children are transferred, at a somewhat earlier physical age (11 or 12) on reaching about the same mental level, to a central or secondary school where the curriculum usually has a definite bias—industrial or commercial in the case of most central schools, classical, scientific, or 'modern' in the case of most secondary schools.

It is, accordingly, eminently desirable that every child, before he leaves the elementary school, should be made the subject of special study, and that he should be recommended to enter, or to train for, that particular type of occupation for which (so far as can be judged) he seems by mental constitution best adapted.

This, however, raises a further question. How far is it possible to estimate a person's vocational aptitude at the early age of 14?

(*a*) It is possible, in the first place, that for certain careers vocational aptitude can, and should, be discovered still earlier. We have, for example, some evidence that this is so for the musical, and possibly the dramatic, professions. And, more generally, it would be extremely instructive to test and observe a given group of children, year after year, from a very early age, to discover when different capacities of vocational significance are first clearly shown, and how constant they remain. As Dr Lipmann has stated in his paper, the growth and development of such aptitudes is a problem singularly neglected.

(*b*) It has been shown by numerous researches that, from the early age of 5 or 6 up to that of 14, a child's intelligence, as measured by his mental ratio, is in most cases fairly constant. There is, however, little or no evidence to prove that this constancy is maintained after the stage of puberty. It is argued by many investigators that beyond the age of about 15 intelligence ceases to develop. It seems essential to enquire whether this holds true of all individuals, particularly of supernormal individuals; and whether, for example, just as the mentally defective reach their mental limit at an earlier age, the supernormal may not go on developing for many years after the age of 15. If so, it is possible that there may be wide differences in the period for which this post-pubertal development may continue; so that the final level attained will depend, not only upon the relative speed of intellectual growth (which alone is measured by the mental ratio) but also upon the age at which growth is at last arrested. In London the first selection of supernormal children is made by the Junior County Scholarship examination at the age of 11; at the age of 13 or 14 it is found necessary to hold a secondary or supplementary examination to discover those who (in the current phrase) bloom late—children, that is, who develop at a rapid or even accelerated pace just at the period when the growth of most children seems to be ceasing.

(*c*) There can be little doubt that, besides these individual variations in intellectual growth, the physiological changes of puberty bring with them many modifications in character, temperament, and general personality. Social qualities such as leadership, aesthetic capacities both for execution and appreciation, interests so diverse as religion, money-making, and love, may now emerge for the first time. How far is this due to the intrinsic changes of puberty, and how far to the sudden change of environment and work? Often, I am convinced, these fresh qualities and novel interests are called into being only by wider opportunities and the stronger stimulus which the entry into industrial life of itself necessarily involves. And frequently in actual practice the most successful form of vocational choice that an individual can get for himself is the cumbrous and precarious procedure of trying one job after another, taking up one interest and then a different, until at length, by a process of trial and error, he chances to hit (more through good fortune than through foresight and self-guidance) upon the very work for which by nature he is most suited. In not a few instances children who at the early age of 10 or 11 have won scholarships to secondary schools with an academic bias have later on shown manual and mechanical aptitudes even greater than their more academic talents; so that, having specialised too early, a first-rate engineer has been turned into a second-rate clerk or schoolmaster. The London Trade Schools, which do not select their candidates until two or three years after the brightest children have been creamed off from the elementary schools, undoubtedly suffer in this way.

It is clear, therefore, that, particularly with persons of superior mental and financial endowments, the diverging avenues to different occupations should not be completely closed at too early an age. In every individual case the recommendations made before puberty should be, wherever possible, reviewed after puberty; and, for general assistance upon theoretical lines, the intellectual and temperamental changes due to adolescence should (as Dr Lipmann has expressly urged) be more specially watched and investigated.

II. WHAT BODY SHOULD UNDERTAKE VOCATIONAL GUIDANCE?

(*a*) At present the predominating element in the choice of occupation is the wish or the whim of the parent and of the child. The selfish parent looks for the first opening that will bring a quick financial return. The unselfish parent cherishes fond ambitions for his child's future advancement. The child is influenced largely by the gossip of another child-friend or school-fellow; and perhaps almost as much by fantasies about the magic power possessed over the world of matter or society by the engineer or the cinema star. Apart altogether from lack of psychological insight, the parent, and still more the child, cannot possibly have the range of information necessary for a satisfactory choice. Much no doubt might be done—much, indeed, has already been done in America—by familiarising both parents and older pupils with the nature of the employments open to them in their district, showing them, for example, the character of the work as well as the nature of the final product; and perhaps by enlisting their sympathy, through the press and other channels, with efforts now being made to estimate impartially the capacities of their children.

(*b*) Where professional advice has hitherto been given, it has come usually from the child's own teacher. The teacher, and particularly the head-teacher, having had the child under his charge for seven or eight years, acquiring, too, considerable experience from constantly judging his pupils' capacities and from constantly recommending them to neighbouring firms (such as most teachers are in touch with), can often, if he cares to take the trouble, offer guidance that is of the utmost practical value. Its value might be increased still further, if, by means of psychological courses, both during his training college period and afterwards, the teacher were made acquainted with the methods and principles of vocational assessment.

Nevertheless, the teacher is an educational not an industrial expert; and of all professions his is perhaps the least in contact with the world of business. The observations that he can make suffer greatly from the limitation which Dr Lipmann has so forcibly warned us against—namely, the artificiality of the conditions. Indeed, I would even claim that in school an experimental test sometimes removes, rather than adds to, the artificiality that Dr Lipmann deplores as so constant a feature of psychological testing.

I cordially agree, too, with Dr Lipmann that the mere psychologist (and, still more, the mere teacher) must carefully avoid fusing and blurring together their detailed findings into some single statement of a supposed 'vocational aptitude.' The final recommendation must be based upon the cumulative evidence from different spheres, all separately recorded as observed, and stated in detail (as he suggests) in a psychographic 'profile,' schedule, or scheme. The adviser or employment-officer, making this final recommendation, should in turn be trained in the correct interpretation of the data supplied by doctors, teachers, psychologists, home-visitors, and those familiar with the state of the labour market and the requirements of industry.

(*c*) If the task is to be carried out upon any general scale, it would seem, at first sight, that the most appropriate body would be some department of the state or municipality. This department might be either (i) the Labour Exchange, (ii) a special government department, like the Industrial Fatigue Research Board, or (iii) the local education authority. Of these three alternatives, the best appears, on many grounds, to be the last. Much of the data necessary, *e.g.* the records of the medical inspection and of the child's progress at school, is in the possession of the local education authority. The school provides a natural place for the necessary testing or examination, and for the meeting of advisory committees. The Care Committee officers who know the home and the parents are usually officers of the local education authority; and the local education authority will also be most directly in contact with institutions which will provide future training.

Under many local education authorities there is already in existence the necessary administrative machinery for a general scheme of juvenile vocational guidance. The machinery is embodied in the 'revised system of after-care and juvenile employment' such as has been adopted in the boroughs of London. Every child, as he leaves the elementary school, is briefly described upon a school-leaving form. The form is intended to give a confidential record of the child's characteristics—of his intellectual and practical abilities, both general and special, of his moral character, of his physical and medical peculiarities, and of the economic and social environment of his home. The essential particulars are copied from this form on to a card; and indexed by the local association of After-Care Committees. With the assistance of the Juvenile Advisory Committee of the Employment Bureaux or some other agency, the child is recommended for an employment as suitable as can be found. Then, with the aid of voluntary visitors and club superintendents, he is supervised until he reaches the age of 18. The results of the supervision—his progress or his decline, his change of occupation, his subsequent education—are all reported and recorded at the office of the District Organiser.

At present the particulars given upon the school-leaving form are filled in by the teachers during the child's last few days at school, often in a hurry, and usually in a vague and indefinite fashion. 'Conduct, good'; 'Intelligence, fair'; 'Will be a credit to anyone who takes him'—these are common types of entry. As testimonials such statements may be helpful; as a scientific guide they are worthless. The list of characteristics and aptitudes, therefore, needs to be drastically revised and defined in accordance with some more scientific 'psychographic scheme'; and the teachers making the records should be instructed in more exact methods of testing or of estimating personal qualifications.

(*d*) It is unlikely, however, that the scientific side of this procedure will be developed by existing public bodies upon their own initiative. For the immediate future, therefore, it seems inevitable that the task of vocational guidance should be taken up by some voluntary agency. There are at present in existence a large number of private offices which profess to offer vocational advice. The majority of these 'consulting psychologists' are utterly unscientific in their methods. The majority find phrenological pretensions the most successful with their clients; a few of the more recent borrow of the jargon of psychological testing and psychoanalysis.

It seems eminently the duty of a National Institute to attempt such a work; and (so far as is possible) to supply, upon a scientific basis, the demand which in certain quarters seems far more widespread than is commonly believed. To this end such an Institute should have local branches established in all large towns, working in close touch with the local education authority, with the labour exchanges, and with the larger businesses and firms.

III. THE ANALYSIS OF THE QUALIFICATIONS NEEDED FOR PARTICULAR OCCUPATIONS.

Vocational guidance obviously implies a study not only of the characteristics of the individuals to be guided, but also of the characteristics of the employments they are to be guided into.

(*a*) The first step would seem to be an inventory of the various employments available in the district in which the work of vocational guidance is to be undertaken. At present the actual recommendations made by teachers or by officers of labour exchanges are based upon vague impressions as to the openings that exist and the qualities that are required. The mere tabulation of current opinions on these points[1] would lead speedily to a provisional working classification of the commoner occupations according to their psychological nature; and, in the absence of more exact knowledge, would be of immediate practical assistance, and would provide tentative hypotheses for exacter research to test. Industrial psychology, like botany and geology, might well begin with careful description and classification. The numerical frequency of the various situations of each particular type would be of much value in directing initial experiments into the most fruitful channels. If more boys enter engineering trades than take up any other employment, if more girls become dressmakers or shorthand typists than anything else, these are the first occupations which call for 'job-analysis.'

It may prove, as Dr Lipmann suggests, that the commonest jobs are sometimes those which have the fewest psychological limitations. To say that "for the majority of men it is indifferent, from a psychological standpoint, into what employment they are directed" is, to my mind, too sweeping. But, certainly, since most jobs require only average abilities, and since most people have average abilities and no more, the task of the psychologist must often be a purely negative one—namely, demonstrating that a given individual is neither subnormal nor supernormal. And it becomes especially necessary—by a study of labour turnover, accidents, differences of output, and so forth—to find, first of all, what employments call for very special qualifications.

(*b*) The following-up of persons (particularly of persons already studied by psychological methods) after they have entered the occupations for which they have been recommended, will of itself throw valuable light upon the qualities needed in those particular employments. The investigator, who is responsible for the recommendations, would, to begin with, have a legitimate ground for appealing to the firm for information. And the firm in turn would willingly report on employees recommended to it who had since proved particularly efficient or inefficient. A small intensive study could then be made of each group with a view to discovering which of the known qualities possessed by the various individuals had been favourable or unfavourable to success.

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[1] It is, however, too often forgotten that the traditional names of various occupations and trade-processes sometimes convey very misleading notions as to their real psychological nature under the conditions of modern industry

(*c*) It is desirable, however, to make, upon a more extensive scale, a scientific study of the qualities required for the commoner trades. The obvious procedure is to apply appropriate tests to large groups of actual workers already engaged; and to correlate the results of the tests with the independent judgments of the foreman or of the manager of the works. With this plan the chief obstacle is the difficulty of obtaining independent judgments which are unbiased and reliable. In some cases, where it is not possible for the firm to rank every individual in order of merit, it may prove easier for it to pick out, on the one hand, workers known to be highly efficient, and, on the other hand, those known to be highly inefficient; correspondence with the test-results will then be measured, not by coefficient of correlation, but by so-called coefficient of colligation[1].

As a preliminary to such investigations in the workshop, experiments may be conducted in industrial training schools, for example, trade schools and polytechnics under the local education authority, apprentice schools attached to the larger businesses, and private schools and colleges for commercial students. This procedure will have several advantages for the first tentative enquiries. In the first place, the necessary interruption of the regular work is more likely to be permitted in the case of the apprentices than in the case of the firm's actual employees. Secondly, the master of the apprentice school or of the trade school is capable of supplying a more trustworthy and impartial ranking of the pupils under his charge than the foreman or works' manager. Thirdly, the masters of such schools have already been compelled to elaborate some rough procedure for gauging the capacity of applicants and apprentices; and are more likely to understand and appreciate the problem. A critical study of the entrance examinations in vogue in apprentice schools would offer valuable suggestions to the psychologist, and evoke a sympathetic welcome from the directors of the schools, who are far from satisfied with the schemes they have evolved.

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[1] Dr Lipmann also urges that the method of *Gruppenvergleich* has been too often neglected for that of *Rangreihenvergleich*.

(*d*) For a thorough and intensive study of the needs for particular trade processes, probably the best method will always be for a trained psychologist to seek temporary engagement as a worker himself, and so study by introspection the nature of the processes and capacities required.

(*e*) To do this for every possible trade and trade-process is a task that would demand an infinite allowance of time. For the urgent needs of the moment, it would seem wiser (i) to choose first of all those occupations for which the largest number of pupils enter, and the largest proportion of employees give dissatisfaction, and (ii) those occupations which are most closely akin to the most easily observable or measurable activities.

IV. WHAT QUALITIES ARE TO BE ASSESSED IN MAKING RECOMMENDATIONS FOR VOCATIONAL GUIDANCE?

(*a*) *Physical and medical.* The assessment of physical and medical qualifications or disabilities should present small difficulty. Every child before it leaves school has been medically examined on at least two or three occasions; and its physical history is usually recorded upon the medical card. Some slight modifications could no doubt be suggested which would increase the vocational significance of the present system of medical inspection; and the school doctor should understand that he is not merely searching for curable defects or ailments, but also recording particulars which will presently be consulted by the Juvenile Advisory Committee when the time comes for the child to be placed in a suitable employment. Every vocational institute should have a qualified medical man upon its staff.

(*b*) *Educational attainments.* Educational attainments can now be measured with a fair degree of exactitude; and are of great importance for a certain limited group of occupations. For the higher professions a high standard of general education is indispensable; without such acquirements even the preliminary examinations cannot be passed. For humbler clerical and semi-clerical positions particular school subjects are specifically required; spelling for shorthand typists, arithmetic for accountants, handwriting for clerks, drawing for draughtsmen, English composition for secretaries. And, even apart from semi-scholastic occupations, and simply as a rough test of general ability, one of the commonest questions for an employer to put is the 'standard' in which the child was working when he left his school. It is not usually understood that the reply to such a question has nowadays far less psychological significance than it had 20 years ago when 'standards' were regulated by the old Board of Education Code. The new generation of teachers, unfamiliar with the obsolete code of regulations, possess but the vaguest notions of what the 'standard' requirements were; and of recent years it has become increasingly the practice to promote children into the higher classes on the ground not of educational attainments but simply of size and age. Indeed, many teachers openly realise that it is a bad recommendation for a child to leave in any standard below the seventh; and, in their generosity, act accordingly. It has, therefore, become highly necessary that attainments should be measured by means of a scientific scale of scholastic tests.

(*c*) Of all psychological capacities, general intelligence is the easiest to measure; it is also the most significant for the purposes of vocational guidance. Hitherto psychological investigation has succeeded in discovering no other general factor, certainly no other general factor with anything like so wide an applicability. In other directions we seem confronted, not with a general power of memory, a general power of attention, or a general power of manual skill, but rather with specific and relatively independent forms of each of these several functions which share a common name. Even if (as upon theoretical grounds, some are inclined to suppose) there really are, underlying these various cognate capacities, certain undiscovered 'central' or 'group' factors, nevertheless no adequate test for them has in any case been established or devised. A certain degree of general intelligence, on the other hand, differing in different instances, is needful—and in theory assignable—for almost every kind of industrial work. More particularly, by its very nature, general intelligence seems closely related to the power to learn—to the ability to adapt oneself quickly and efficiently to new tasks and situations. Hence, for any trade which does not demand mere repetition of routine movements, or the mere reiterated application of knowledge and skill already acquired, general intelligence must be fundamental. In view, therefore, of the present difficulty of measuring other mental qualities, I am inclined to suggest that for the time being the chief (though not the sole) provisional guide in making vocational recommendations should be the degree of the candidate's intelligence.

Here two obvious principles will probably command almost an immediate—indeed, almost too ready an acceptance. (*a*) First, as a principle of guidance, other things being equal, a candidate should be always recommended to aim at the highest form of employment of which his intelligence is capable. (*b*) Secondly, as a principle of vocational selection, out of a given batch of candidates the most intelligent will always give the most satisfaction. And here it might be thought that the value of intelligence tests would end. In practice, however, certain important reservations must be added to each of these broad principles. In questions of vocational guidance, besides the candidate's general intelligence, his interests, his special qualifications and his financial means will frequently still further restrict his range of choice. Secondly, in questions of vocational selection, an employer does not sufficiently realise the importance of a complementary principle, namely, that it is possible to engage an employee who is too intelligent for his job, as well as one who is too dull. I have been told of a factory where there were trade-processes of two different levels of difficulty, the one dull and mechanical, consisting mainly in the minding of machines, the other demanding alertness and resource, and consisting mainly in the arranging and packing of delicate articles in fancy boxes—the last stage of the manufacturing process before the commodity came into the customer's hands. It was sufficiently obvious that, upon work of this latter type, an unintelligent girl would not only be slow and incompetent, but possibly spoil half the material that she handled. It was not equally obvious that the bright and active girl put on to dull mechanical work would become irregular and discontented, and either lapse into inattentive day-dreaming, or disturb her fellow-workers by the overflow of her superfluous energy into joking, giggling, and chattering. Both groups were tested; and the brightest girls on the dull job were transferred to the more difficult, and the dullest girls on the more difficult job were transferred to the more mechanical. Thus, without dismissing a single employee, and by a simple interchange of tasks, output and quality of work were alike enormously increased. It is probable that for most occupations there is an optimal range of intelligence with an upper limit as well as a lower[1]. One of the main tasks, therefore, of vocational guidance will be this: to draft each child into a job which corresponds as precisely as possible with his own level of intelligence, and requires neither a greater nor a less amount of mental capacity than he actually possesses.

A survey of London school children indicates that the whole population might be divided according to their intelligence into eight sections or grades.

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[1] I am grateful to find Dr Lipmann stressing the same point: and urging that we have been too eagerly concerned about 'minimal requirements' to the neglect of what he has also named 'the upper limit.'

1. The highest section includes the few children with mental ratios above 150. These are rarely found in elementary schools; and are almost entirely confined to the families from the higher social and professional classes. Among the general population they probably number less than 1 per mille. They constitute the type of pupils who win not only scholarships to secondary schools, but also scholarships to universities and honours degrees.

2. Secondly, there are children whose mental ratio is over 130 but under 150. This group comprises the top 1 or 2 per cent. of the elementary school population. The majority of them later on win scholarships to secondary schools, but to no higher college or university.

3. Children with mental ratios between 115 and 130 are in London usually drafted to schools now designated 'central,' but known formerly as 'higher elementary.' This group comprises rather less than the next 10 per cent.

4. The next section is a large one; and includes all who rise above the mean level of the whole, but who do not attain a mental ratio of more than 115. It forms 38 or 39 per cent. of the total.

5. The fifth section is equally large; and includes all who fall somewhat below the mean level of the whole, but again by no more than 15 per cent. of their age. This similarly contains nearly 40 per cent. of the total; and together these two sections constitute the dense medium of moderate ability, nearly four-fifths of the whole.

6. The sixth section includes those whose mental ratio lies between 85 and 70. These correspond roughly with what are now known as the 'dull and backward.' In an industrial area like London or Birmingham they form rather over 10 per cent. of the whole school population.

7. School children whose mental ratio falls below 70 are usually during childhood certified as feeble-minded; and committed to a special school. On reaching the age of 16, however, the majority of them are in practice virtually de-certified, and pass out into the world to earn their own living, if they can, like other children. They constitute rather more than the bottom 1 per cent. of the total school population.

8. The last group comprises those who are ineducable and will remain for ever in an institution—imbeciles and idiots with a mental ratio below 50. They constitute about 1 per mille of the child population.

Is it possible to arrange the occupations of adults, by strata, as it were, in any corresponding series? A provisional answer to this question may be sought in three different directions. First of all, there are the data collected by the American War Department which tested during the war a vast number of recruits, and classified the resulting performances according to occupation. Secondly, there are in this country the data obtained through the testing of candidates for Civil Service posts; these examinations, carried out during the past two or three years, have now covered nearly 40,000 candidates; and the range of ability displayed is almost as wide as that which would be found in a small group of schools, ordinary, central and secondary. Thirdly, isolated adults, many of them falling above or below even this wide range, have been tested in this country for various purposes—a small but increasing number of them by the National Institute, definitely for vocational guidance.

The results so collected tend to show that adults may also be classified into eight parallel groups, corresponding with the grade of their vocational work.

1. There are, first of all, adults, for the most part highly educated, and either following the higher professional careers (University lecturers, lawyers, medical men), or holding the higher administrative posts either in business or in the service of the State. These have a mental ratio[1] of over 150—averaging about 165.

2. Secondly, there are educated adults with lower professional qualifications (*e.g.* elementary teachers), and clerks holding higher and more responsible posts. These have a mental ratio usually over 130, averaging about 140. The intelligence of this group thus corresponds roughly with that of the secondary school child.

3. Thirdly, there are clerks of a lower grade doing for the most part work of an intelligent, but moderately routine, character. Workers engaged on highly skilled labour have also an intelligence at least equal to this group. These have a mental ratio averaging about 125; and usually over 115. The intelligence of this group thus corresponds roughly with that of the central school child.

4. Those engaged upon skilled labour usually have a mental ratio over 100 but seldom rising above 115. To the same group belong many of the ordinary commercial posts—successful shopkeepers and tradespeople on a small scale, and shop-assistants employed by larger firms.

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[1] With adults of more than average intelligence a mental ratio is not directly calculable, since hardly any test-scales provide, or could provide, mental ages above 16. I believe that a far more scientific measure of intelligence is one expressed in terms either of the standard deviation or of percentiles (which, with a normal distribution, are convertible units). For the sake of simplicity I have expressed my marking throughout in terms of a mental ratio, basing my calculation on the assumptions that an individual's mental ratio and percentile position are constant throughout life.

5. Fifthly, there is the large mass of semi-skilled labour, whose mental ratio falls below 100 but seldom sinks below 85.

6. The sixth category comprises unskilled labour. This category resembles in general level, though in actual numbers it somewhat exceeds, the dull and backward type of child. The mental ratio of the members comprising it falls usually below 85.

7. Seventhly, casual labour of the poorer types, and the lowest class of domestic service and of rural labour, show an intelligence little if at all above that of the high-grade feeble-minded child, and a mental ratio which is often well below 70.

8. Finally, there are the defective adults certified for institutional care. The mental ratio of most of these falls below 50.

The foregoing parallel is put forward rather as a rough but concrete illustration than as a scientific deduction from an exhaustive survey. As enumerated above, the vocational categories overlap enormously; nor is there sufficient data as yet available to define with any exactitude the limits of each class. It would seem, however, eminently desirable, even with our present imperfect knowledge, to work out, for any district in which a broad scheme of vocational guidance is attempted, the percentage of occupations available under each category.

For London a rough calculation (based mainly upon Charles Booth's survey, corrected by later census figures) gives the following proportions:

Vocational category	Proportion of the total population per cent.
I. Higher Professional	0·1
II. Lower professional	3
III. Clerks and highly skilled workers	12
IV. Skilled workers. Most commercial positions ...	27
V. Semi-skilled labour. Poorest commercial positions	36
VI. Unskilled labour, etc.	19
VII. Casual labour, etc.	3
VIII. Imbeciles and idiots	0·2

It will be observed that the proportions for the vocational categories show a fair correspondence with those for the different intellectual categories among the school children. But the distribution is less symmetrical. The percentages in the lower groups are too large, and those in the higher groups too small, for a perfect correspondence.

(*d*) The next group of qualities calling obviously for assessment consists of specific *vocational attainments and capacities*. A few of the persons who have come to us for vocational guidance have been older children or young adults who have already undergone a certain measure of vocational training. A girl, for example, of 16 has been attending a private school which professes to train children for clerical posts. Her parents asked whether she is now fit to apply, with any hope of success, for a post as a shorthand-typist. Here the answer will be largely determined by a direct 'sample' test of her actual attainments in these two directions. The task of vocational selection, as distinct from vocational guidance, is often simplified in the same manner. The employer requires a shorthand-typist who is already capable of a certain degree of speed and accuracy in shorthand and typewriting. The same holds good of a large number of other posts. The firm wants to know, not whether the candidate is suited by nature for taking up dressmaking or engineering, but whether his training or experience is already sufficient to enable him to be profitably engaged.

In some instances of selection, however, and in most instances of guidance, the task is not so simple. What really has to be estimated is not the candidate's present attainments but his apparent aptitudes, his capacity for acquiring the necessary attainments after future experience or apprenticeship at his job. In selecting children for an apprentice school, or in offering advice to children who are leaving the elementary school, it would be useless to try a 'sample' test of shorthand and typewriting or of engineering skill. We have to test native ability rather than acquired knowledge or dexterity, not previous accomplishments but future educability. Here, therefore, there lies a difficult but urgent field for psychological research. To test attainments is usually easy. To test capacity for acquiring those attainments is not only a difficult thing in itself, but is rendered still more precarious by the fact that children vary enormously in the degree of knowledge, information, and experience which, to however small an extent, they may already have obtained. It is impossible, for example, to test a girl's aptitude for dressmaking by giving her a needle and thread and seeing how quickly she can learn to sew a given type of seam. Some have already been trained at home or at school in the use of a needle; others, perhaps, have never sewn a single stitch.

(*e*) This brings us to the general task of devising tests for *special abilities*. We need to know what abilities are specific, how specific they are, and into what trade processes they severally enter. How far, for example, is manual dexterity dependent upon general intelligence? If independent of general intelligence, how far is it common to different kinds of manual work? And of what particular industrial activities does it form an essential component?

The testing of specific abilities seems of special importance in the case of the two middle categories—the dense medium of average ability. We have seen that over 75 per cent. of the population have a mental ratio between 85 and 115, thus varying but little from the general mean. If we are to offer to children from this group any definite advice whatever, it is clear that we must do far more than merely assess their general intellectual level.

Here I agree with Dr Lipmann that vocational psychologists have too readily assumed the existence of what are variously called, according to the fashion of the moment, 'faculties,' 'specific capacities,' or 'group-factors.' So-called partial correlation may in the future do much to demonstrate the existence, or non-existence, of special group-factors. Meanwhile, I agree that for such *centralen Eigenschaften*, particularly for those which he terms 'qualitative,' observation is, as a practical guide, perhaps even more trustworthy than testing.

The cross-classification of occupations, according to the special abilities required by certain types or groups, will lead to a yet further stratification of employments according to their psychological needs.

(*f*) Vocational advice, however, is not to be offered merely after an estimate of the abilities of the child. *Temperamental qualities* are equally important. Here, indeed, is yet another vast and difficult region demanding exploration. Tests of temperament as yet hardly exist. There are, it is true, several experimental methods which are sufficiently suggestive to be worth investigation. The psycho-galvanic reaction, the associative reaction, the arrangement of pictures or words according to their emotional appeal (Pressey test), the power to modify certain habitual activities such as handwriting (June Downey test), the answering of questionnaires by the candidate or his teachers and parents as to his neurotic and emotional history—all these suggest methods which may perhaps be standardised and made to yield a numerical assessment of temperamental qualifications.

For the present, however, it seems clear that in assessing temperament we must have recourse to the older of the two main scientific methods, namely, that of observation[1] rather than of experiment. And here we might commence with investigations upon the reliability and accuracy of the traditional methods of making observations upon temperamental peculiarities. How far, for example, do physiognomy and the personal impressions of an oral interview form trustworthy guides? How far can the judgments which they yield be improved by such devices as the use of a rating-scale, or of a statistical scheme of marking based perhaps on the assumption of a normal distribution of the qualities to be assessed? In gradually forming an opinion as to the character of a stranger, what proportion of the new information is given by first impressions, and how far are the first impressions of one observer correlated with those of another?

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[1] Upon this point especially I am in accord with Dr Lipmann in emphasising the importance of observing the child's spontaneous behaviour in natural conditions, as distinct from merely testing him under the artificial conditions of a short psychological experiment.

These, then, are the main qualities which need to be assessed in any case of vocational guidance. There are still numerous other problems which await solution and even formulation; for example, the relative merits of group testing and individual testing for various purposes, the relative merits of speed tests and qualitative tests[1], the relative merits of linguistic and non-linguistic material. These further problems have already been discussed in the literature of general testing; but I doubt whether the experience gained by the more recent vocational researches has as yet anything fresh to offer. Enough, perhaps, has been said to raise certain problems for general discussion.

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[1] Our experience is, on this point, at one with Dr Lipmann's. The observations made during a test are usually quite as suggestive as the mere numerical test-score; and, hence, where feasible, *Einzelprufung* is usually preferable to *Massenprufung*. For preliminary surveys, and for selection as opposed to guidance, group-tests have often the advantage.

THE PRINCIPLES OF VOCATIONAL GUIDANCE (III)[1]

BY L. L. THURSTONE.

BEFORE presenting my list of principles, let me define two or three terms which I shall be using.

By the 'criterion' I mean the most direct statement that can be obtained of the trait which the test is intended to measure. If a test is intended to measure ability in salesmanship, then the actual measure of performance in selling would constitute the criterion for a test in salesmanship. The independent variable in such a study would be the scores in the test of salesmanship and the dependent variable would be the criterion, which would be the direct measure of performance in selling.

The 'diagnostic value' of a test is indicated by the correlation between the test scores and the criterion. The 'predictive value' is the same as the diagnostic value with this difference—that the predictive value refers to a criterion which is to be obtained by future performance, whereas the diagnostic value is the correlation with a criterion which can be obtained at the time the test is taken. 'Diagnostic value' and 'validity' are synonymous terms.

The 'reliability' of a test is a concept which, in my opinion, is very much confused. It is supposed to measure the reliability of the test score itself, but I shall point out some difficulties in the use of that concept.

The following are some of the principles of vocational testings:

1. In order for a test to be justified, it must be easier to ascertain the test score than the criterion. If this condition does not obtain, the mental test serves no useful purpose. The test is used mainly because it is easier to get the test score as an index of the criterion than to get the criterion itself.

2. No test should ever be used for anything unless it has been checked against a criterion.

This principle seems so obvious that it should hardly be necessary to state it, and yet the principle is not infrequently violated. No test should ever be given unless it is given to test something. That something is the criterion. It is of course necessary to establish first of all that the test actually does check with the criterion before it can be released for practical use in measuring the criterion.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July, 1923.

3. The criterion should be objective whenever possible.

4. The criterion for a vocational test must reflect relative ability and not experience.

It is important to keep this principle in mind when a vocational test is being checked with a group of people who vary considerably in experience. There may be in such a group a number of people of considerable experience who gain high ratings in the criterion on account of experience rather than on account of ability. The subjects who have recently entered the occupation may have superior ability and they may gain low ratings in the criterion on account of inexperience. If the test is intended to measure present vocational skill it is legitimate to allow differences of experience to influence the test score. If, on the other hand, the test is intended to measure vocational aptitude, it would obviously be illegitimate to allow differences of experience to affect the test score. Most vocational tests are intended to measure aptitude or promise. Such tests are devised to select people for entry into an occupation and it is therefore essential that the criterion be a measure of aptitude and ability rather than experience.

5. A mental test can not be said to be a good test except with reference to a definite criterion. A test may be good for one criterion and poor for another criterion. A test is, therefore, never by itself good or bad. It is always good or bad with reference to the criterion that the test is supposed to diagnose. There is no excuse for devising a mental test unless it is devised and checked against a criterion.

6. A mental test can never serve as a criterion for another mental test. If a new test is shown to agree well with an old and well-known test, this fact does not prove that the new test is as good as the old test. The only way to prove the diagnostic value of a new test is to check it against the criterion for which it is intended. The fact that a new test correlates well with some other test which in turn correlates well with a certain criterion does not prove that the new test can be used for diagnosing that criterion.

A number of recently devised tests have been checked against the Army Alpha intelligence test and the fact of a high inter-test correlation has been presented to show that the new tests were good tests. The fact that a new test agrees well with the Army Alpha test, which has been extensively standardised and used, does not prove that the new test is a good one. The only way to show that a new test is useful is to check it directly against a criterion.

If we consider the inter-correlations of three variables, two tests and the criterion, we find that it is possible for the two tests to have a fairly high inter-correlation and for the two tests to vary considerably in their correlation with the criterion. Therefore a high inter-test correlation can not be cited as proof that the new test will correlate well with the criterion. In other words, one mental test can never serve as a criterion for another test.

7. Judgments about people's proficiency should refer to specific things that they do or fail to do and about which different judges would agree fairly well. One should avoid using for the criterion any judgments which are general and vague and about which different judges would readily disagree.

8. The criterion measure must always be acertained in such a way that the test score has no influence on it. If the criterion consists in judgments or rating scale estimates the judge must be ignorant of the test scores of the people that he is judging. If this condition does not obtain there is introduced a fictitious relation between the test score and the criterion which would not be verified in the practical use of the test.

9. The criterion range of the correlation table that is used as an index of the diagnostic value of a vocational test should be comparable with the range of abilities of applicants who seek entry into the vocation. If the criterion range is smaller than the range of abilities of applicants, the correlation will be lower than it should be to indicate the diagnostic value of the test. If the criterion range is greater than the range of abilities of applicants, the test is assigned a diagnostic value which is fictitious and too high. The range of abilities in the general population is always greater than the range of abilities among those who apply for admission to the vocation. Since the vocational test is to be used in selecting people who apply for admission to a vocation, it is proper that the correlation table which is used to indicate the diagnostic value of the test should represent the range of abilities of applicants. If the correlation table represents people who are already in the vocation, the indicated diagnostic value is too low. If the correlation table represents a sample of the general population, the indicated diagnostic value of the test is too high.

An example will make the above principle clear. If we admit students to college partly on the basis of an intelligence test and if we want to determine the diagnostic value of an intelligence test for such purposes, we give the test to a group of applicants, admit them to college, and then note what happens. We determine the correlation between the tests and scholarship. If we use a group of students who are already successful in college work in order to determine the diagnostic value of the test, the criterion range will be smaller and the diagnostic value correspondingly low. On the other hand, if we should give an intelligence test to a sample of the general population and check the test scores against the real or potential scholarship of such a sample, the correlation would be very high. The diagnostic value so indicated would, however, be high because the groups of applicants with which the college administrator would be dealing are not random samples of the general population. The correlation table on which the administrative value of a test is judged should represent the range of abilities which is typical for the people on whom the test will actually be used. Stated briefly, the principle is that the criterion range in the correlation table which is used to determine the diagnostic value of a test should be comparable with the range of abilities of the people on whom the test will in practice be actually used.

10. The diagnostic value of a test is indicated by the correlation between the test and the criterion.

11. A mental test may have diagnostic value at one end of its scale and no diagnostic value at the other end of the scale.

12. The normality or skewness of a distribution of test scores does not indicate anything regarding the diagnostic value of a test for any criterion.

13. The diagnostic value of a mental test is demonstrated better by fair agreement with the criterion in ten groups of one hundred than by fair agreement with the criterion in one group of a thousand.

14. For the purpose of diagnosing the criterion by means of a mental test, the regression of the criterion on the test is of fundamental value, whereas the regression of the test on the criterion is of little administrative value. The reason for this is that the test is in practice to be used as a diagnosis of the criterion. The criterion is never used as an index of the test score.

Much of the confusion about mental age has come about because of a violation of this principle. In standardising Binet tests we select a group of ten-year old children and give them a lot of tests. Their average performance is considered a twelve-year old performance. Their average performance is a point on the regression line of test on age. In the practical use of the test the regression is reversed. The test score is known and the age is diagnosed. In that case we should use the other regression, namely the regression of age on test. If we had not been using the wrong regression line in the Binet tests, we should not now have the general confusion about mental age, particularly the paradox about the adult population being fourteen years old. A better plan in using Binet tests would be to assign a sigma standing or percentile rank to the child to indicate the comparison of its mentality with other children of the same chronological age. Such a procedure would avoid the present misunderstandings about adult mental age.

15. If the scoring method for a test has been determined by selecting the scoring method which gives the highest correlation with the criterion in a single sample, it is necessary to repeat the try-out on a new sample with the predetermined scoring method in order to ascertain whether the optimal scoring method remains constant. This must be done before the test is accepted for practical use.

I have come to this conclusion after several failures. I have given mental tests to a group of people and then juggled the scoring method in order to make the test agree as well as possible with the criterion. On several occasions I have used multiple correlation methods to accomplish this. When I turned about to use the test I found that the optimal scoring method would not stay put. For this reason I declare it as a principle in vocational test procedure that one complete try-out on a fresh group of subjects must be carried out with a predetermined scoring method before the test is turned over for practical use. If the scoring method of the test has been modified as a result of the last group on which the test was tried, it indicates that still another try-out is necessary before the test is ready for administrative use.

16. If a group of tests has been selected from a large number of tests, on the basis of multiple correlation work, to constitute a vocational test programme, it is necessary to try the programme with the predetermined selection of tests and the predetermined scoring methods on a new group of subjects in order to ascertain whether the diagnostic value of the test programme remains constant. This should be done before the test programme is announced for practical use.

I enunciate this principle as the result of a number of failures in my own work in which I have used multiple correlation methods to select the individual tests for a test programme. I have found that it is possible to 'boost' the multiple coefficient of a group of tests with a criterion if one has plenty of tests, even if none of the zero order coefficients exceeds ·2 or ·3. The practical difficulty with this procedure is that the multiple correlation does not remain in its high value when the same test programme is applied to a fresh group of subjects. For this reason I should insist on this fundamental principle of vocational testing, that when a group of tests has been selected from a large number on the basis of multiple correlation procedure, it is necessary to try out the whole test programme with a predetermined selection of tests and predetermined scoring methods on a fresh group of subjects in order to verify the practical value of the test. If it is found necessary to juggle the selection of the tests further and to alter the scoring methods in order to retain the same diagnostic value, the test programme is not yet ready for administrative use. Most of the studies in vocational testing stop after the first try-out of the scoring methods. Few vocational test programmes are investigated far enough to make them ready for administrative use.

17. The reliability of a test is increased by increasing the number of independent questions.

18. The reliability of a test question is increased by increasing the number of alternative answers for each question.

19. A test which consists of 'yes-no' answers, or 'plus-minus' answers, should contain more questions than a test in which there are more than two alternative answers in order to retain the same reliability of the test score.

20. The proper length of a test can be determined by correlating separately the first tenth, the first two-tenths, the first three-tenths, the first four-tenths, etc., of the test, with the criterion. Such a procedure will show that the correlation between the test and the criterion increases with the length of the test. The length of the test should be extended until little is gained in diagnostic value with the criterion by increasing the length of the test further.

21. If the correlation between the test and the criterion is plotted against the length of the test, we have a convex upward function. The rate of increase in diagnostic value of a test decreases with the increase in the length of the test. In other words, more is gained in diagnostic value by the first ten questions of a test than by the last ten questions of a test.

22. The reliability of a test is always higher than its diagnostic value. Therefore, if the diagnostic value is demonstrated to be acceptable, it is unnecessary to determine the reliability of the test itself.

My impression is that some investigators in this field consider it almost more important to determine the reliability of a test than its diagnostic value. This I believe is the reason why some investigators will never prepare a mental test of any kind unless it is prepared with parallel forms to determine its reliability. It is theoretically desirable to know that the test score itself is reliable, but it is much more important to determine what the test shows. If the test is shown to have a certain correlation with a criterion it necessarily follows that the reliability of the test is always as high or higher than the correlation between the test and the criterion. Therefore it is not necessary to determine the reliability of a test if its correlation with a criterion is high enough to be acceptable. If a test fails to agree with a criterion, it is theoretically possible that the failure is due to the unreliability of the test itself, but this possibility is in practice rather small if the test is arranged in the short-answer form with a fairly large number of independent questions. Much time and labour can be saved in this field by eliminating the great emphasis on the reliability of tests and concentrating more on the diagnostic value of the tests with the criteria for which the tests are devised.

23. The probable error of the correlation between a mental test and a criterion is useless for administrative purposes.

In practical work with mental tests the actual correlations fluctuate much more than is indicated by the probable errors of the coefficients. The actual fluctuations of the coefficients are about five or ten times greater than would be expected from the probable errors of the coefficients even when the coefficients represent groups that are for administrative purposes entirely comparable.

It is essential to keep in mind that the fluctuations which are caused by chance are very much smaller than the fluctuations which are actually found in successive groups which may reasonably be called comparable. The probable error concept has been overworked in the mental test field, and it is not sufficiently generally realised that the probable error affords control over one of the least important factors that cause fluctuation. The fluctuations due to chance are very much smaller than the fluctuations which are due to other causes in comparable groups.

24. There are several different ways of determining the reliability of a mental test, and these different ways do not give the same results. If alternate questions in a test are correlated, the reliability is found to be relatively high. If the first half is correlated with the second half, the reliability is lower. If two alternate forms of a test are correlated, the reliability is found to be still lower. If the same test is given to the same subjects on two different occasions, the reliability is a different figure.

25. Vocational selection is easier than vocational guidance.

26. The vocational counsellor should not make a vocational decision for a student. The student should make that decision for himself. If the student makes high scores in vocational aptitude tests, the counsellor can say that the student will probably succeed. If the student makes

low scores in the tests, the counsellor can say that success would probably be obtained only by very great effort, or that success is doubtful. In the latter case the student's interest in the vocation should be balanced against his talent for it in giving advice.

27. In the development of a mental test the known abilities of the subject are used as the standard for testing the value of the test. In the actual use of the test this relation is reversed, in that the test is used to predict or diagnose the abilities of the subject.

28. A vocational test should be tried out on applicants for a vocation or on people who have recently entered the vocation. The test records so obtained should be filed and later checked against vocational performance in order to determine the diagnostic value of the test.

29. It is legitimate to try a proposed vocational test on people who are already in the vocation provided that the content of the test does not include anything that is acquired by experience in the vocation. The diagnostic value of the test is not finally verified until it is shown that it predicts vocational ability when given at the time of entry into the vocation.

30. When starting to devise tests for any special ability, it is better to begin by studying successes and failures than by studying random samples. If random samples are used in the first studies, the investigator wastes much time on the records of the intermediate subjects for those tests which could easily be shown to be useless by working only with a few successes and a few failures. This method of group differences should of course be followed by studies of random samples of applicants for those tests which show a difference between the successes and the failures. The tests that show no differentiating value for the successes and failures can be discarded in the study of group differences. A test which shows no group differences between successes and failures can not possibly show any correlation with the same criterion in a random sample of applicants.

31. No vocational test programme can be complete unless it provides opportunity for an estimate of the social, emotional, and temperamental characteristics of the subject and their relation to the social requirements of the vocation in question.

32. It is not necessary to define intelligence or any special abilities in order to measure them. It is only necessary to demonstrate that the tests actually diagnose the abilities, or that the tests predict the products of the abilities. The fact that the tests actually work is sufficient basis for using them. Definitions of the traits measured are not necessary as a basis for using the mental tests.

33. Vocational tests in early adolescence have the serious limitation that the interests change markedly during adolescence. The special abilities such as those for mathematics and for music can probably be safely diagnosed at an early age. The tests for intelligence are probably fairly stable with increasing age. On the other hand any vocational test which is intended to measure the traits of personality must be used with caution because of the great changes in these traits during adolescence.

It has not been my intention to present a complete list of principles. I have only summarised in the form of generalisations some of the conclusions which I have reached in working with mental tests of various kinds. I believe that it would be well for other investigators in this field to stand back from their immediate data to formulate principles of vocational testing. I believe that progress in this field will be gained more rapidly if we plan our studies as far as possible to discover and establish principles of vocational testing, rather than to demonstrate the diagnostic value of particular tests for particular groups.

VARIATIONS IN THE MENTAL EFFICIENCY OF CHILDREN DURING SCHOOL HOURS[1]

By SHEPHERD DAWSON.

I. *The problem* (pp. 362–363).
II. *The method of procedure* (pp. 363–364).
III. *Results* (p. 364).
IV. *Conclusions* (pp. 364–369).

I.

THERE are many considerations which determine the arrangement of a school time-table, *e.g.* the time to be devoted to each subject, and the organisation of the staff, but there is one of special importance, namely, the most effective placing of the work-periods. The most difficult or most important subjects are usually placed at those hours in which the mind is supposed to be at its best.

The popular opinion appears to be that this is about half-an-hour or an hour after the beginning of the day's work, for during the period of rest the thinking machinery has been allowed to run down and the first half-hour is spent in getting it once more into running order; then, as the work progresses, fatigue slowly sets in and becomes more and more acute until at the end of the day the efficiency of the organism is at its lowest ebb.

This opinion, in so far as it is not the result of casual observation, appears to rest mainly on the results of laboratory experiments, and on observations of the work of factory operatives. The direct application to school practice of generalisations based on such experiments and observations is to be deprecated for the conditions under which school work is done are very different. School-boys are not nowadays submitted to the dull monotony which characterises much factory and

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laboratory work: hence laboratory and workshop experiments, in which the work is long-continued and of uniform difficulty, are far removed from the normal conditions of school life. It was to find how the rate of working varies under actual school conditions that the following experiment was performed.

II.

The experiments were performed in schools in the west of Scotland by the teacher in charge of a class in the following manner. On the blackboard were written several arithmetical questions, numbered (1), (2), (3), etc., in each of which a number of three digits was to be multiplied by another also of three digits, *e.g.* 756 × 348. The questions were so framed that each digit occurred as often as any other and no more than once in each question. 0 and 1 were excluded. More questions were put than could be answered by the quickest of the children, and the pupils were instructed to work as quickly as possible, paying no attention to the neatness of the work, for the essential thing was to answer as many questions as possible in the time allowed.

At a word from the teacher the children copied a question from the blackboard, worked it, copied the next, and so on, for ten minutes; then the papers were collected. The same performance was repeated on each of the following five days, a different set of figures being used each day. The work-periods were placed at different hours on different days, namely, at 9.30 a.m., 10.30 a.m., 11.30 a.m., 1.30 p.m., 2.30 p.m. and 3.30 p.m.

To distribute the effects of practice equally over the whole series, the children who performed the experiments were divided into five nearly equal groups, A, B, C, D and E. Those in group A began with a 9.30 period on Monday, followed by a 10.30 period on Tuesday, 11.30 on Wednesday, 1.30 Thursday, 2.30 Friday, and 3.30 Monday. Group B began with a 2.30 period on Monday followed by a 9.30 period on Tuesday, and so on, as shown in Table I.

TABLE I. *Showing the distribution of the work-periods.*

Group	Mon.	Tues.	Wed.	Thurs.	Fri.	
A	9.30	10.30	11.30	1.30	2.30	3.30 Mon.
B	2.30	9.30	10.30	11.30	1.30	3.30 Tues.
C	1.30	2.30	9.30	10.30	11.30	3.30 Wed.
D	11.30	1.30	2.30	9.30	10.30	3.30 Thurs.
E	10.30	11.30	1.30	2.30	9.30	3.30 Fri.

It will be seen that, with the exception of the 3.30 hour which came last in every series, the effects of practice were uniformly distributed over the work periods.

In order to make the assessment of the work done as accurate as possible each correct multiplication of two digits was given a mark and each addition of one column was also given a mark, for example, in multiplying 753 by 9 the total possible was 3 because three separate multiplications by 9 had to be performed. The columns to be added consisted of one, two or three figures: marks were given only for the two and three-figure columns. Where two successive errors were made in multiplication or addition, the teacher used his discretion in deciding whether the second was merely the effect of the previous error or involved a new one: in the first case a 'correct' mark was given, in the other it was called an error No serious difficulties arose here.

III.

The records of each child gave four values, the number of (1) multiplications correct, (2) additions correct, (3) errors in multiplication, and (4) errors in addition. The records of children of the same age were added together and the averages calculated. These are shown in Table II. Since nothing was gained by keeping them apart, the marks for multiplication and addition have here been added together as have also the errors. The grand total is shown in Table III. The number of operations correct measures the effective work, the number of errors measures the ineffective work, while their sum might be taken as a measure of the gross output. The number of children in each group is shown in Table IV. It was nearly constant throughout the series. The groups A, B, C, D and E, though intended to be so in the scheme of the experiment, were not equal. The age-groups 9–10, 10–11 and 11–12 were the biggest and numerically not very dissimilar.

IV.

Let us consider first Table III which gives for children of all ages the average number of correct operations performed in each of the six periods, the probable errors of these means, the differences between the 9.30 mean and each of the others, and the probable errors of these differences. The average error is very nearly the same at each period, with perhaps a slight tendency to increase toward the end of the morning and afternoon sessions and with the amount of work done. The variations are, however, so irregular that it was considered that

TABLE II. *Showing average number of correct operations and errors of each age-group.*

Work-period		9.30	10.30	11.30	1.30	2.30	3.30
Age 8–9	C.	59·19	58·80	52·11	51·72	53·31	57·67
	E.	4·19	4·60	4·35	4·10	4·34	4·17
	No.	100	96	100	94	100	89
Age 9–10	C.	80·30	84·55	77·27	75·49	74·75	87·72
	E.	2·83	3·36	3·54	3·17	3·39	3 42
	No.	283	284	284	284	284	273
Age 10–11	C.	95·58	102·55	97·16	93·38	87·19	107·02
	E.	3·03	3·30	3·89	3·21	3·23	4·27
	No.	340	339	340	339	337	333
Age 11–12	C.	116·13	128·31	124·13	121·86	124·08	128·33
	E.	3·04	3·65	3·17	3·82	2·98	3·47
	No.	274	276	288	286	285	258
Age 12–13	C.	126·52	141·95	137·43	135·28	138·87	146·02
	E.	4·10	4·51	5·01	4·54	5·19	4·54
	No.	144	147	146	147	146	123
Age 13–14	C.	152·16	162·13	162·35	142·49	138·95	170·57
	E.	3·38	4·22	4·30	4·01	3·83	4·78
	No.	59	59	53	59	59	59

C. is the average number of correct operations.
E. is the average number of errors.
No. is the number of children in a group.

TABLE III. *Showing the average number of right and wrong operations, for all groups.*

Work-period		9.30	10.30	11.30	1.30	2.30	3.30
Correct	Mean	100·13	108·47	102·90	100·17	101·69	110·93
	P.E.$_m$	±0·798	±0·932	±0·909	±0·875	±0·917	±0·931
	D.$_m$	—	8·34	2·77	0·04	1·56	10·80
	P.E.$_{dm}$	—	±1·227	±1·208	±1·180	±1·210	±1·226
Errors	Mean	3·24	3·69	3·83	3·61	3·56	3·93
No. of children		1200	1201	1211	1209	1211	1135

P.E.$_m$ is the probable error of the mean.
D.$_m$ is the difference between the 9.30 mean and each of the others.
P.E.$_{dm}$ is the probable error of this difference.

TABLE IV. *Showing the number of children in each group.*

Age	A	B	C	D	E	Total
8–9	11	8	7	43	31	100
9–10	22	70	45	109	37	283
10–11	33	90	88	88	41	340
11–12	43	61	63	61	46	274
12–13	35	31	39	13	26	144
13–14	11	11	31	0	6	59
Total	155	271	273	314	187	1200

the calculation of the probable errors would not be worth the trouble spent on them.

The mean rates of working at the different hours, which are given in the first row of the table, show that, with the exception of the 10.30 and 3.30 periods, the output was practically constant throughout the day. The differences between the 9.30 mean and the means of the 11.30, 1.30 and 2.30 periods are probably not significant for each is less than three times its probable error. The difference between the 9.30 and the 10.30 mean is much more than three times its probable error and is therefore significant, in fact, on the assumption that this is only a chance variation, the probability of getting a difference of ± 8 or more on repeating the experiment in only 1 in 223,400, or, in statistical language, the odds against the 10.30 mean and the 9.30 mean being non-significant are over 223,400 to 1. The odds against the 3.30 difference (10·8) being non-significant are over 151,000,000 to 1.

The reasons for these significant differences appear when we examine the conditions under which the experiment was performed. The 3.30 period differed from the others in coming last in the series, *i.e.* after the children had had five periods of practice: this suggests that the 11 % increase on the 9.30 performance was the result of this practice. To test this suggestion the records were re-grouped so as to put together those of all the students on the first day, then those obtained on the second day, and so on. This re-grouping gave the following average number of correct and incorrect operations:

	Right	Wrong	No. of children
First day	87·63	3·68	1212
Second „	100·91	3·10	1201
Third „	103·79	3·66	1204
Fourth „	108·76	3·65	1213
Fifth „	112·30	3·84	1202
Sixth „	110·93	3·93	1135

The gain from practice is obvious. There was, in fact, a steady increase in the number of correct operations except on the last day. This gradual increase and the slight drop on the sixth day (the 3.30 hour) suggest that the 11 % increase above referred to is not due to an 'end-spurt,' *i.e.* to special effort induced by the prospect of concluding the series.

There does not appear to be in the records any information to explain the slight fall in the average of the sixth day. It may have been due to the length of the interval between the fifth and sixth work-periods, which was longer than that between any other pair of consecutive periods, or to slackness induced by the prospect of completing the series (such a prospect is as likely to slow down the work as to speed it up), or finally it may indicate a general falling off in efficiency at 3.30.

Similar results were given in another set of experiments in which children of ages seven to ten years worked simple addition 'sums' each consisting of two columns of digits each of five rows. The procedure was in all essentials the same as in the multiplication experiment. The average marks are lower than in the multiplication series because one mark was given for each column correctly added, and this was a longer operation than each of those which earned a mark in the other experiment. The average numbers of correct and incorrect operations were:

	Right	Wrong	No. of children
First day	10·53	2·02	868
Second „	10·66	2·07	859
Third „	11·27	1·96	857
Fourth „	11·70	2·08	860
Fifth „	12·30	2·19	853
Sixth „	11·44	2·50	750

Both experiments, then, show that the gain from practice is very large and far outweighs the effects of any other condition.

The increase of output (8 %) at the 10.30 period seems to support the popular opinion that the best working hour of the day is the second. An examination of the detailed results, however, shows that it is not due to any efficiency of the hour, but mainly, if not entirely, to the influence of routine. The tradition that the second hour of the day is the best for work is so strong that, on consulting the time-tables of the schools in which the experiments were performed, I found that 77·3 % of the schools and 77·5 % of the scholars had their arithmetic lesson about 10.30.

This suggested that the superior output at 10.30 might be due, not to the hour as such, but to the fact that the exercise was done in the hour that was normally devoted to arithmetic. To test this hypothesis I collected the records of all those whose arithmetic lesson was normally at another hour: they are shown in Table V.

TABLE V. *Showing the average marks and errors of those who were unaccustomed to doing arithmetic at* 10.30.

Work-period	9.30	10.30	11.30	1.30	2.30	3.30
Correct Errors	83·2 4·2	81·4 4·8	75·7 5·3	76·9 4·2	78·3 4·2	91·9 5·1
No. of children	271	266	267	267	273	270

The increase at 10.30 has disappeared. This is all the more striking because the 10.30 hour, and to a slightly less extent the 9.30 period, had a distinct advantage over the others on account of the unequal distribution of the 270 children among the groups A, B, C, D and E. The numbers in these groups were (A) 20, (B) 39, (C) 55, (D) 105 and (E) 54. A glance at Table I will show that the 10.30 period was fifth in the series for those in group D, which was the largest, hence, with the exception of the 3.30 period, the 10.30 period has an advantage in respect of the amount of practice that preceded it.

It, therefore, seems legitimate to conclude that the superior output of the 10.30 period, as shown by the grand total of Table III, is due mainly, if not entirely, to the fact that this is the usual arithmetic hour. An attitude had been developed which favoured the performance of arithmetical operations at this hour. The importance of these habit-attitudes is confirmed by reflection on everyday experience. It is easier to catch the train by which one is accustomed to travel than to catch an earlier or even a later one. We adapt ourselves to a time-table, we take the task for granted and settle down to it, whereas any disturbance of the normal routine is apt to be distracting: we are happiest in familiar surroundings doing the things we are accustomed to do and in the way we are accustomed to do them.

Let us turn now to the more detailed records of Table II. Each age-group except the 8–9 group shows the same practice effect at 3.30. Even in this group, which is somewhat small, the effect is present, but obscured. It happened that the numbers in this group were (A) 11, (B) 8, (C) 7, (D) 43 and (E) 31, making 74 in the last two groups as against 26 in the others. The chronological order of the tests for group D was 11.30, 1.30, 2.30, 9.30. 10.30 and 3.30, while for group E it was 10.30, 11.30, 1.30, 2.30, 9.30 and 3.30, which in respect of practice gives the advantage to the first periods of the day. This group, then, also shows the practice effect.

It is significant, however, that the 3.30 mean of the 8–9 group is less than the 9.30 or the 10.30 mean. Similar results were given by the other set of experiments in which children of ages seven to ten worked simple addition sums. These means are shown below (Table VI).

The drop in the 3.30 mean may be due to any of the causes already suggested, viz. the lateness of the hour, disinclination to work induced by the prospect of finishing the series, or the length of the interval between the 2.30 period and the 3.30 period Further expriment is necessary before a decision can be given; but the fact that in the 7–8 and 8–9 groups there is not the same rise in the 3.30 mean as with the older children suggests that for children below the age of nine there is a distinct falling off in mental efficiency in the later hours of the school day.

TABLE VI. *Showing the average number of correct additions and errors.*

Work-period		9.30	10.30	11.30	1.30	2.30	3.30
Age 7–8	C.	10·20	11·38	9·57	9·61	10·40	10·25
	E.	1·82	2·11	1·72	2·16	1·99	1·61
	No.	259	263	262	263	259	215
Age 8–9	C.	11·91	12·15	11·35	11·37	11·17	11·82
	E.	2·05	2·05	2·16	2·15	2·23	2·47
	No.	399	393	391	390	392	347
Age 9–10	C.	12·79	12·77	11·95	12·07	11·75	13·51
	E.	1·82	2·35	2·09	1·80	2·31	2·82
	No.	204	204	207	205	205	188

C. is the average number of correct additions.
E. is the average number of errors.
No. is the number of children in a group.

Table II shows that the average number of correct operations of each age-group is less at 1.30 than at 11.30, and it might be inferred that there is a reduction of mental efficiency after the mid-day interval. Table III, however, shows that there is little statistical foundation for such an inference, for the average difference between these means (2·73) is less than three times its probable error (± 1·26).

The results of this experiment have shown, then, that the mental efficiency of school children over nine years of age, as measured by the rate at which they perform simple arithmetical operations, is approximately constant from 9.30 till 2.30 p.m.; that the effect of a small amount of practice in such operations is comparatively large; and that working to a time-table leads to the formation of habit-attitudes which have a pronounced and beneficial effect. As the 3.30 hour came last in all the series, we cannot say whether there would have been any falling off in efficiency at this hour if the effects of practice had been distributed. There is, however, some evidence of a reduction in the mental efficiency of children below nine years of age after a five hours' day.

It is hardly necessary to remark that these generalisations are based on the results of experiments with only one kind of work, and that they *may* not be true of other kinds, but somewhat similar experiments[1] on such operations as memorising words or numbers and marking errors of spelling seem to indicate that they are true also of other forms of mental activity.

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[1] Cf. Thorndike, E. L., "Mental fatigue," *Psych. Rev.* 1902, VII, 466–482, and 547–579, and Bolton, T. L., "The growth of memory in school children," *Amer. Journ. of Psych.* 1892, IV, 362–380.

FUNCTIONS OF THE SIZE OF INTERVAL IN THE SONGS OF SCHUBERT AND OF THE CHIPPEWA AND TETON SIOUX INDIANS

By HENRY J. WATT.

Introduction (pp. 370–372).
Schubert's Songs (pp. 372–378).
Chippewa and Sioux Songs (pp. 379–384).
Summary and Conclusions (pp. 385–386).

In 1907 Dr C. S. Myers stated that "in folk music generally the frequency with which the various intervals are used decreases proportionately with their size[1]." And this rule or the psychological function underlying it seemed on extensive and detailed examination[2] to be the fount and origin of a large part of musical practice. It seemed to me desirable to increase our knowledge of this primary aspect of melodic structure by statistical means.

As promising material I chose first the songs of Schubert. A statistical study of the intervals of melody presupposes really melodic songs. And probably the surest test of melody is acceptance of the songs in which it is embodied by a cultured musical public. It could hardly be disputed that Schubert's songs fulfil these conditions. And they are as numerous as they are beautiful. They belong, moreover, to a culminating period in the development of our complex music. They are in no sense tentative or experimental in their melodic build. Thus they should form an interesting counterpart in many qualities to the usages of primitive music. An important consideration was also that I had access in the library of the University to the Kritisch durchgesehene Gesammtausgabe of Schubert's Works, published by Breitkopf and Härtel. This edition comprises 567 songs for the voice written during the years 1811 to 1828. It seemed to me that a sample of these, consisting of every tenth song, would be fairly representative of his style and would yield as much statistical material as was needful. But on closer consideration I decided to exclude such songs as contained recitative or as were not songs for a single voice. Recitative differs of course considerably from the explicit melody of song. And it is to be presumed that it may differ from the latter in the various functions of the different intervals. So I chose the nearest, and in the first place, the following song that had no such undesirable feature. Where there were two versions of a song I took the second. In the majority of cases the whole song was analysed. The songs studied were the following: 8, 20, 30, 41, *50*, *60*, *70*, *80*, *90*, *100*, *111*, *120*, *130*, *140 b*, *150*, *160*, 171, 180, *190*, 200, 210, *220*, 230, *240*, 251, 260, *270*, 280, *290*, 301, 310 *b*, *320*, 331, *340*, 350, 361, 371, *380 b*, 391, 400, 410, 420, 430, *440*, 450, 460, 470, 480, 490, 500, *510*, 520, 530, 540, 550, 560. Of the songs whose numbers are printed in italics only one verse was used, as the others were merely repetitions of it.

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[1] "The Ethnological study of Music." Among *Anthropological Essays presented to E. E. Tylor*, Oxford, 1907. Cf. E. M. von Hornbostel, "Phonographierte tunesische Melodien," *Sammelbände d. internat. Musikges.*, 1906, VIII, 41: "Dass die Frequenz der Intervalle im umgekehrten Verhältnis steht zu ihrer Grosse, scheint ein allgemeines melodisches Gesetz zu sein, das fur Orientalische Melodien (v. Abraham u. v. Hornbostel, "Phon. türk. Melodien," *Zeitsch. f. Ethnologie*, 1904, XXXVI, S. 208 f.) eben so gut gilt, wie für Europaische" (v. H. Rietsch, *Die deutsche Liedweise*, 1904, S. 102, 111). I have not access to the latter work.

[2] My *Psychology of Sound*, Cambridge, 1917, Chap. VI, and *The Foundations of Music*, Cambridge, 1919, p. 205 (as summary).

After having analysed these 56 songs[1] and found the results somewhat faulty and less complete than desirable, I repeated the analysis by the following method. On the oblong sheet of a stock ledger I entered at the top of the columns the names of the intervals in their order of size: p. 2, II, 3, III, 4, T, 5, 6, VI, 7, VII, 0. These symbols were also placed in a column down the left-hand side of the page, where two lines were allowed to each of the intervals 2, II, 3, and 4, these being far more frequent than the other larger ones. The lower line of the two was reserved for intervals that followed the interval (of the top horizontal line) in question by *contrary* motion. Ascending intervals were entered on the left half of the appropriate square, descending intervals on the right half. A similar arrangement was possible for the larger intervals within the square of a single line. Before making these entries I wrote out the whole series of intervals of the song on successive lines on the left of the sheet (provided in the ledger for the description of stock), using one line for each passage of melody bounded by pauses[2], and placing the interval below the line if it descended, above the line if it ascended. This procedure ensured accuracy. By using the first perpendicular column for intervals that began a passage and the last horizontal one for those that ended a passage, each sheet could be cross-checked. The sheets then showed for any interval of the top horizontal column the number of times it was followed by any, and by which, ascending or descending interval in similar or in contrary motion. In this way I hoped to obtain data regarding various aspects of the use of intervals in melody.

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[1] The frequencies given in my *Foundations of Music*, p. 34, note, are the results of this working.

[2] Intervals over pauses were not counted as intervals.

In summarising the results I divided the 56 songs into groups that were meant each to comprise 14 songs. By mistake, however, the first group contains only eleven songs: the other groups include 17, 14 and 14. But this error is quite immaterial, as will be evident in the tables below. So I did not trouble to correct it.

TABLE I. *Schubert's Songs* (56).

Frequencies of the different intervals in four consecutive groups of songs. Expressed in percentages of the frequencies (II) of the major second (=100 %).

Group	p	2	II	(II)	3	III	4	T	5	6	VI	7	VII	0
8–111	59·1	70 4	100	(399)	45·3	15·3	28·5	3·2	8·7	2·7	3·0	0·7	—	—
120–280	52·6	67·3	100	(437)	35·0	17·9	22·2	2·0	12·1	5 0	2 3	1·8	—	1 6
290–420	61·4	78·0	100	(559)	39·9	18·2	27·0	5·0	6·8	6·1	1·4	0·5	—	0·7
430–560	98·1	84·5	100	(772)	53 0	27·7	35·1	1·4	8·9	6·6	3·6	0·4	—	2·4
430–560	73·6	63·4	75	—	39·7	20·7	26·3	1·0	6·7	5 0	2·7	0·3	—	1·8
All	72·2	76·8	100	(2167)	42·7	21·0	29·2	2·8	9·0	5·4	2·7	0·8	—	1·4
Minor	109·0	107·6	100	(553)	57·0	20·1	31·6	2·9	12·9	4·5	2·0	0·5	—	1·8
Major	59·7	66·2	100	(1614)	37 7	21·3	28 3	2·7	7·7	5·8	2 5	0·9	—	1·1
Possible major	42·6	165	100	—	47·1	35·5	23·6	13 5	6 4	1·0	0·3	0·1	—	0·8
Calculated 60 %	—	166	100	—	60	36 0	21·6	13·0	7·8	4 7	2·8	1·7	1·0	0·6
Calculated 47 %	—	213	100	—	47	22·1	10·4	4·9	2·3	1·1	0·5	0·2	0·1	0·1

The figures of this table are percentages expressed in terms of the frequency of the major second on the same line, the absolute frequency for which is given in brackets on the right in the column headed (II).

Table I displays the relative frequency with which the different intervals are used in the four groups of songs in terms of the frequency for the major second, which is set down as 100 %. The absolute frequency for the major second is given in each case in the column on the right in brackets. It will be noticed that the percentages are not only very similar, but also vary very similarly in the four groups from interval to interval. Only in the fourth group is the frequency of 2, 3, III and 4 rather higher than in the other three groups. The large number of excessive frequencies in this group suggests that the primary discrepancy lies in the frequency of the major second and not in those of the larger intervals. If all the percentages for this group are reduced by 25 %, as is done in the line next below in the table, much closer agreement with the percentages of the other three groups of songs is obtained. This means that in the fourth group the major second has been used relatively less frequently than in the other groups, viz. by 33 % of its frequency in the fourth group. The direction of change of frequency from one interval to the next, however, is independent of this. Thus the summary for the 56 songs represents fairly and clearly the usages throughout this series of songs. The frequency of usage of any interval decreases rapidly as the interval exceeds a major second. A distinct reversal of the rule appears only at the intervals of the fourth and the octave and possibly at the fifth and the sixths. This appears to confirm the statement found in text-books of musical composition that large intervals enter more easily into melodies when they are such as form consonances with simultaneous tones.

This result suggests a rule that has much else to recommend it, namely that an interval is used in some inverse proportion to its size that would appear perfectly regular if we could abstract from our data the influence of consonance and of other factors that may have modified them. Of the latter the most obvious is the frequency with which each interval occurs in the scales used by Schubert—the major and minor diatonic scales. But the frequencies for these two scales are very different. The only course, therefore, was to find out what proportion of the numbers for each interval was used in minor passages and what proportion in major passages.

The result of this enquiry is seen in the next two lines of Table I. Here again the rule of relative frequency stated above is maintained except in the case of the minor second in minor keys, for which a relative frequency of 107·6 % is reached. The minor and major tonalities are used by Schubert in these songs in the frequency of 553 to 1614 major seconds or 25·5 to 74·5 %. For the four successive groups of songs the percentages of minor passages to the whole were 19·3, 27·3, 23·1 and 52·1. The basis of most of the songs is major. A large number of the occurrences of the minor second in minor keys must therefore be set down to the act of transition from major to minor rather than solely to the greater melodic cogency of the small interval. It would thus be hazardous to accept the relations between minor second amd major second frequencies in these minor passages as truly representative of their relative frequencies in basal minor melodies. But this objection will not apply to the major passages of these songs to any disturbing degree. The process of modulation will probably affect their number. But the value of modulation need not lie entirely in the change to another key. It may be partly due to the greater melodic cogency provided by the larger number of minor second intervals it makes available. So major passages including modulations may be taken at least as an obvious type of melodic material. And they may be used as a closer approximation to the ideal frequency afforded by the major scale pure and simple than is what we have in the mixed major and minor music of these songs.

A perfect material that could be used as a basal standard would have to be free of all modulations.

The ideal frequency should be approached from the figures of Table I by adjusting each percentage frequency shown in the line "Major" by its reciprocal frequency in the major scale relatively to the frequency therein of the major second. The intervals 2, II, 3, III, 4 and T occur respectively 2, 5, 4, 3, 6, times and once in the major scale, as do also their inversions VII, 7, VI, 6, 5 (and T). Therefore to correct the percentage 66·2 for the relative frequency of the minor second, since the latter has a possibility of only 2 to that of 5 for the major second, we multiply the minor's frequency by five upon two, making it thereby 165. And so for the other intervals, whereby the line "Possible Major" of Table I results.

The figures in the next line, "Calculated 60 %," show how nearly a ratio of 60 % decrease of frequency for each semitone of increase of interval meets the figures in the line above ("Possible Major"). The only notable exception is the minor third where there is a defect of 13 %. The percentage 47, on the other hand, which is the frequency of the minor third, better suits the frequencies of the far off intervals 6, VI and 7. If we adopt it—and there is no apparent reason why the frequency of the minor third should be disturbed, unless it be by the process of modulation—then we should infer that the excessive frequency (relatively to the ratio of 47 %) of the intervals III, 4, 5, and 0, is the result of their high degree of consonance. And it would concord with this that the greater degree of consonance gives a greater *proportionate* disturbance, viz. III—1·3 times (65·5 ÷ 22·1), 4—2·1 times, 5—2·8 times, octave—more than 6 times. Then it would also appear that the minor second is not used so frequently while the tritone is used more frequently, than it should be by this calculation.

It is not easy to choose between these alternatives on the data of Table I alone. Many theorists, I fancy, might be unwilling to surrender the influence towards greater frequency commonly ascribed to the consonances. It is no doubt an old doctrine, and as the direct appearances of the line "All" of Table I suggest, a useful one. But in so far as it claims to be the expression of a real function of consonance, it must stand scrutiny without prejudice in its favour. For the nonce I feel inclined to accept the calculation that meets so many of the actual frequencies of the line "Possible Major" and to leave a simple query against the 'abnormal' rarity of the minor third. Reduced frequency amongst the intervals greater than the fifth could be rationalised without great difficulty if on a very much more extensive count the discrepancies of their frequencies showed themselves to be really significant. For the data of Table I we can hardly assume that they are. And it would be a happy sign of simplicity if the influence towards increase of frequency of the consonances turned out to be illusory.

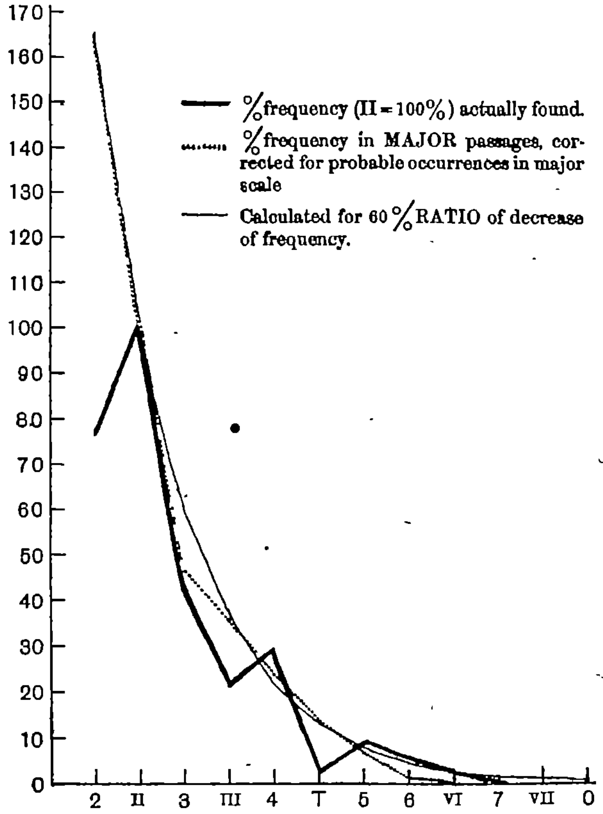

Fig. 1. (Schubert's songs.)

But there can be no doubt of this that as the interval grows in size so does its frequency of use diminish by a proportion approximately constant for each increase of a semitone. In Fig. 1 appear graphically (1) in the broad line the percentages of occurrence in Schubert's songs of the several intervals (*v.* line "All" in Table I); (2) in the dotted line the percentual frequencies for the major passages of these songs, corrected for the probability of occurrence of each interval in the major scale (line "Possible Major" in Table I); and (3) in the thin line the frequency calculated from the standard of 100 %, adopted throughout Table I for the interval major second, with the help of the ratio 60 % of decrease from one interval to the next a semitone larger (line "Calculated 60 %" in Table I).

Ascent and descent. In order to obtain Table II first of all were found the relative frequencies of ascent and descent for each class of interval in the four groups of songs and in the other divisions shown in the column on the left. These relative frequencies were expressed in percentages of the frequencies of ascent. The results for the major second are shown in brackets. Thus 143 major seconds descended for every 100 that ascended. Then these percentages for every interval on any line of the table were converted into percentages of the percentage of the major second, which therefore became 100 %. So each line of the table shows the relative frequency of descent of the various intervals in relation to the relative frequency of descent in the case of the major second. There is evidence of fair regularity in each group and in "All' together, showing that as the interval increases, it is used more frequently for ascent than for descent. Descent is made more frequently by the smaller intervals than by the larger ones, except in the case of the tritone and the great consonances of the fifth and the octave. In the minor second the tendency to ascend increases relatively to that of the major second.

TABLE II. *Schubert's Songs.*

Percentual frequencies of descent relatively to ascent (=100 %) for the different intervals: expressed in percentages of that of the major second (II)=100 %.

Group	2	II	(II)	3	III	4	T	5	6	VI	7	VII	0
8–111	53·1	100	(143)	82·5	81·1	51·0	13·0	105	7·7	49·6	14·0	—	—
120–280	83·9	100	(180)	65·0	71·7	21·1	55·5	118	38·3	6·1	7 8	—	66·6
290–420	67·4	100	(175)	57·1	30·9	263	51·4	57·1	18·9	28·6	—	—	—
430–560	66·6	100	(195)	45·1	37·9	40·0	11·3	70·8	23·6	51·3	0	—	29·7
All (56)	67·8	100	(176)	59·1	52·8	36·3	132	83·5	33·0	34·7	17·6	—	42·6
Absol. freq. of ascent	(760)	(785)	—	(455)	(236)	(386)	(18)	(80)	(74)	(36)	(13)	—	(16)
Minor	72·3	100	(202)	39·4	38·2	28 4	67·8	76·7	13·0	495·8	24·8	—	49·5
Major	63·1	100	(168)	70·2	60·1	41·4	185	82·1	41·1	20·4	16·2	—	35·7
Calc. 82 %	—	90	—	73·8	60·5	49·6	40·7	33·4	27·4	22·4	18·4	15·0	11·3
Differences	—	+10	—	-3·8	-0·4	-8·5	+144	+48·7	+13·7	-2·0	-2·2	—	+24·4
% out	—	+11	—	-4·8	-0·6	-17·1	+354	+146	+50	-8·9	-12·0	—	+216

For example, in the groups 8–III 143 major seconds descend for every 100 that ascend. This % 143 is then taken as 100 %. So that % frequency of descent in the minor second was 143 × 53·1 ÷ 100 = 76.

This general tendency becomes clearer when we separate the frequencies for the minor passages from those for the major passages. In the former the rule is still apparent: only there is a very sudden drop in the percentage from the major second to the minor third. The major parts show a much more regular fall of frequency from the major second to the fourth with a similar rise over the fifth, falling again thereafter. If we venture to calculate on the basis of a regular difference of 82 % from step to step (*e.g.* 90 × 82 ÷ 100 = 73·8) we see that thereby six (five) of the values are met very closely, viz. II, 3, (III), 4, VI, and 7. (The percentage was pivoted on the major third approximately.) The discrepancy is very great only for the tritone, the fifth, and the octave. These intervals are evidently used far more often for descent than their size alone would warrant. The excess percentages shown in the lowest line of Table II indicate a greater excess for the octave than for the fifth and an enormous excess for the tritone. The excess of + 50 % for the minor sixth is distinct. The defect of − 17·1 % for the fourth is in keeping with the notorious frequency with which this interval is used ascending as the beginning of a melody. For this reason an appearance of melodic primacy attaches to it[1], which the general consideration of the use of intervals in melodies shows that it does not possess.

Similar and contrary motion. The data for this important aspect of melodic process are shown in Table III, which is constructed in exactly the same way as Table II. In order to reveal the actual frequencies involved, those of similar motion for all the songs together have been given in the sixth line. The upper part of the table shows the results for intervals that precede any given interval, the lower part for intervals that follow. The frequencies in the different intervals for preceding motion are rather different from those for succeeding motion; but they are very similar in their direction of change in each step of a semitone upwards, especially as far as the interval of the fourth. Beyond the fourth the absolute numbers of cases are probably too small to bring out in a regular way any slight tendency to uniformity they may involve. We must be satisfied with the fact that the percentages are all large. Subtraction of the frequencies found in minor passages makes little difference to the percentages, so they have not been entered in the table.

It is interesting to notice how the intervals are related to oblique motion. Here the percentage in brackets (16·7) expresses the frequency of oblique motion relatively to the combined frequencies of similar and contrary motion for the major second in relation to all preceding intervals no matter what their size. The changes of frequency from one interval to the next larger show some similarity, but are not nearly so distinct. The first maximal point is at the fourth.

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[1] Cf. O. Sterzinger, *Archiv. f. d. ges. Psychologie*, 1916, XXXVI, 14.

TABLE III. *Schubert's Songs.*

Percentual frequencies of contrary relatively to similar motion (=100 %) for the different intervals; expressed in percentages of that of the major second (II)=100 %.

A. *In relation to the* PRECEDING *interval.*

Group	2	II	(II)	3	III	4	T	5	6	VI	7	VII	0
8–111	122	100	(78·0)	177	199	691	∞	705	1154	∞	∞	—	—
120–280	107	100	(59·7)	179	143	792	670	557	368	586	335	—	∞
290–420	136	100	(79·4)	126	132	239	117	83·0	2519	∞	∞	—	—
430–560	151	100	(67·5)	166	117	477	1037	255	1728	1778	∞	—	1630
All (56)	133	100	(70·7)	157	134	445	310	273	1061	1366	849	—	2263
Absol. freq. of similar motion	(651)	(1050)	—	(343)	(175)	(100)	(16)	(47)	(10)	(3)	(1)	—	(1)
Oblique	131	100	(16·7)	116	131	224	23·4	190	98·4	186	597	—	246
B. *In relation to the following interval.*													
8–111	108	100	(73·6)	227	505	414	679	∞	∞	∞	—	—	—
120–280	170	100	(38·0)	602	227	537	789	2718	2369	921	∞	—	—
290–420	146	100	(56·2)	322	199	395	2224	1779	2847	∞	∞	—	—
430–580	184	100	(52·2)	273	194	287	∞	1571	1389	230	383	—	670
All	158	100	(53·6)	379	224	367	1586	2164	1972	496	2799	—	∞
Absol. freq.	(691)	(1112)	—	(242)	(158)	(157)	(6)	(10)	(7)	(12)	(1)	—	(6)
Oblique	122	100	(15·8)	104	151	158	33·2	125	257	143	0	—	1082

Chippewa and Teton Sioux.

As these results seemed promising, I sought for a collection of more or less primitive songs to form a distant counterpart to them. The 183 Chippewa songs published by Miss Densmore in *Bulletin* XLV of the Bureau of American Ethnology in 1910 seemed very suitable, and I made a statistical analysis of them. It was not until this analysis was laid out that I found that Miss Densmore herself had made some statistics of these and other Chippewa songs in Vol. LIII of the Bureau. Fortunately my data are a little fuller than hers, so that my work has not been quite wasted. I used—as in Schubert's songs—all parts of the melody that were different, so excluding all obvious repeats as not constituting another, or a new, melody. These statistics for the Chippewa songs may be compared with those extracted by Miss Densmore in Vol. 61 of the Bureau for the Teton Sioux, whose songs she divides into two groups—an old and a relatively modern one.

TABLE IV. *Chippewa and Sioux Songs.*

Frequencies of the different interval, expressed in percentages of that (II) of the major second (=100 %).

Nos. of different intervals used (Nos. of songs)	2	II	(II)	3	III	4	T	5	6	VI	7	VII	0
2 (12)	—	100	(74)	93·2	9·5	8·1	—	—	—	—	—	—	—
3 (57)	0·8	100	(379)	96·3	36·4	16·8	—	2·9	—	0·3	—	—	—
4 (47)	7·6	100	(331)	98·1	35·6	19·3	—	7·2	0·6	0·3	0·3	—	—
5 (47)	27·4	100	(314)	100·6	54·4	29·3	—	11·8	0·6	0·6	—	—	1·0
6 (13)	31·1	100	(161)	55·3	21·7	25·5	1·2	13·1	—	1·9	0·6	—	—
7 (3)	26·1	100	(23)	87·0	21·7	43·5	—	8·7	—	4·3	—	—	—
8–10 (4)	6·8	100	(132)	69·1	13·6	13·6	—	9·1	—	1·5	—	0·88	6·8
All (183)	12·7	100	(1414)	90·1	34·8	20·7	0·1	7·6	0·3	0·7	0·14	0·07	0·9
Calculated 60 %	166	100	—	60	36·0	21·6	13·0	7·8	4·7	2·8	1·7	1·0	0·8
Densmore (340)	12·7	100[1]	(3089)	84·9	31·5	26·2	8·2		1·9		0·3		1·4
Sioux Old (147)	15·8	100	(1749)	72·3	23·6	30·8	6·5		1·6	0·6	0·7		1·8
Sioux New (93)	13·4	100	(1154)	73·0	17·5	35·4	5·5		0·7	0·6	0·1		2·7
All the Indian (580)	13·2	100[2]	(6275)	78·0	25·5	28·9	7·1		0·6	1·2	0·3		1·7

The last four lines are reckoned from figures given by Miss Densmore in Vol. LIII, p. 25 and Vol. LXI, p. 33.

The first part of Table IV, which is constructed like Table I, comprises the frequencies extracted by me from the 183 Chippewa songs of *Bulletin* XLV and shows prevailing regularity. With the exception of the minor second an interval is used the more frequently the smaller it is. The only exceptions to this in the first seven lines for intervals less than the octave are percentages for the minor third (100·6) and for the fourth (25·5 and 43·5), if we ignore the two exceptional tritones. The percentages in each column are generally of a similar order of size: they show a peculiar upward trend from the first and the seventh lines in each column towards the fourth line, and for the intervals minor second and fifth towards the fifth line. This indicates that the songs using five intervals show the slowest decrease of frequency with increase of interval. In songs using especially a smaller number of intervals the fall in frequency with increase of interval is the more rapid the smaller the number of intervals used. The usage of intervals tends on reduction of the number of different intervals used to crowd about the major second and the minor third. Songs using more than five intervals show less characteristic and regular behaviour.

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[1] Miss Densmore's figure is for the "Second" 3481. Later (in Vol. LXI, p. 331 she tabulates this figure as that for the "Major Second." But the figures she gives for the Sioux are very like my figures for the Chippewa, and the latter did contain numerous minor seconds. So I have reckoned the percentages for Miss Densmore's 340 songs as if there were 12·7 % of minor seconds in them, *i.e.* taking 3481 as equal to 112·7 %, 100 % being 3089, difference 392.

[2] Similarly here, 392 was deducted from the frequency of the major as given by Miss Densmore (6667) and added to that of the minor second.

"All the Indian Songs" include a few not included in the three preceding lines of data.

Judged by the "Calculated" ratio of reduction of 60 % for each step of interval, which was found best for Schubert's songs, the frequencies of these 183 Chippewa songs are very nearly right for the major third (error − 1·2 %), for the fourth (− 0·9 %), the fifth (− 0·2 %) and the octave (+ 0·3 %). The frequencies for the sixths and the sevenths are too low, much lower than those given in the line "All" in Table I for Schubert's songs. This is surely in conformity with the fact that this Indian music is primitive in character.

So the only frequency that really calls for explanation is that of the minor third. Miss Densmore found that the average number of semitones in all the intervals of her set of 340 songs was 3·1, just a little larger than a minor third (*Bull.* LIII, p. 26). At first glance this seems significant. But we really cannot tell what meaning it may have until we compare it with averages from other types of music. For Schubert's songs the average size of all the intervals up to and including the octave is 2·765 ± 1·45 semitones. But in the Chippewa songs the percentual frequency of the minor second is only 12·7, whereas in Schubert's songs it is 76·8. And if we reduce the frequency in Schubert's songs to 12·7, thus eliminating 1388 minor seconds from the total number of intervals used, the average size of the interval rises to 3·262. No doubt in a primitive music the place of a number of these minor seconds would have been taken by some slightly larger interval. We can make no safe assumption as to this. But if, for example, we merely suppose them all to have been major seconds, the average size of interval then becomes 2·985, very nearly 3, the minor third. The average size of interval on the assumption of a 60 % ratio of decrease with each semitone is 2·48 ± 1·41 semitones. It must be evident that so general an average can have no value as a characteristic of any type of music.

Probably the most decisive fact indicating the prevalence of the minor third is just the excess of its frequency shown in Table IV over the calculated frequency. For we see that the figures for Miss Densmore's 340 songs (amended in the seconds, *v.* note, Table IV) keep close to the calculation for the intervals III, 4, and 5, but are much in excess for the minor third. Her results seem to indicate some increase of the fourths at the expense of the thirds. This is still more so in the songs of the Teton Sioux, especially in their newer songs. Here the major third seems to lose most in favour of the fourth. Whether the differences between the percentages for Miss Densmore's 340 songs and those of my analysis (183 songs) are due to the other 157 songs included in the former or to my neglect of all repetitions of melodies or unitary parts of melodies is not evident. But there is substantial agreement over and above these differences.

Although the minor third is used so frequently, the percentage of songs in major tonality is decidedly greater than that in minor tonality according to Miss Densmore's distinction of them. Now tonality in primitive non-harmonic music is a very difficult problem, and I have not yet succeeded in finding satisfactory objective criteria of it in these songs. But it is worth while noting that the 60 % ratio to which the Chippewa songs approximate on my analysis is the ratio derived from Schubert's songs from the major passages after due correction for the number of times each interval occurs in our major scale. This would suggest that the Chippewa in general make use of the different intervals (with the exception of the minor third) without regard for any tonality, but approximately in view of their size only. Or, it may be, if more or less distinct traces of tonality occur in their songs, yet when all are slumped together, they blend into an average so as to render these traces of tonality invisible in the results. On the other hand the old Sioux songs give frequencies for the major third and fourth that are much more like the frequencies found in Schubert for the major scale (Sioux—23·6 and 30·8, Schubert—21·3 and 28·3). The newer Sioux tend more towards the minor frequencies (Sioux—17·5 and 35·4 against the Schubert minor passages 20·1 and 31·6). Miss Densmore's frequencies for these two intervals in the Chippewa songs, finally, are rather more like my own and like those of the 60 % ratio than they are like the major or minor Schubert values (viz. 31·5 and 26·2 to 34·8 and 20·7 and to 36 and 21·6 than to 21·3 and 28·3 and to 20·1 and 31·6 respectively).

It is of interest to note that of the 183 songs there are six that do not use any interval smaller than the minor third.

The percentages for the minor second in Table IV are consistently low. It is evident that the interval is still comparatively new in West Indian music. As Table IV shows, its frequency rises rapidly as the number of intervals used in the songs increases up to six, when it reaches 31·1 % of the frequency for the major second, as compared with the percentage of 12·7 over all the 183 songs. In my analysis the minor second occurs only in 60 of these songs. It is notable also, that, as

Table V shows, the minor third occurs in a just larger number of songs than the major second. This holds slightly more definitely for those songs that employ only two or three different intervals. This table also shows a decrease of frequency for the use of any given interval in songs as the interval in question increases in size. This holds with slight exceptions for each group of songs and more distinctly for all the songs together. Down to the interval of the fifth graphs of the percentages in the various lines of the table indicate a slightly convex rapid fall. The table suggests that the Chippewa first acquired control of the minor third, and then of the major second. As the number of intervals used in songs increased, the next larger intervals rose towards 100 % usage. Intervals beyond the fifth are comparatively rarely used.

TABLE V. *Chippewa Music.* (*Bulletin* XLV.)

Numbers of songs in which each interval occurs, according to the numbers of different intervals used in each song, expressed in % of the number (II) using the major second = 100 %.

No. of intervals	2	II	(II)	3	III	4	T	5	6	VI	7	VII	0	0+
2 (12)	—	100	(10)	120	10	10	—	—	—	—	—	—	—	—
3 (57)	1·9	100	(54)	104	59·3	50	—	1·9	—	—	—	—	—	—
4 (47)	21·3	100	(47)	97·9	70·2	59·6	—	42·6	4·4	2·2	—	—	—	—
5 (47)	67·4	100	(46)	97·8	82·6	84·8	—	58·7	4·3	4·3	—	—	6·5	4·3
6 (13)	92·3	100	(13)	100	100	92·3	7·7	84·6	—	15·4	7·7	—	—	—
7 (3)	100	100	(3)	100	66	100	—	66	33	33	33	—	—	66
8 (3)	100	100	(3)	100	100	100	—	66	—	66	33	33	66	33
10 (1)	100	100	(1)	100	100	100	—	100	100	100	—	—	100	100
All (183)	33·9	100	(177)	101	70·2	64·4	—	36·1	3·4	5·1	1·7	0·6	3·4	3·4

Ascent and descent. Table VI shows the percentages of the frequencies of ascent and descent for each interval. The divisions of the table correspond to those of Table IV. The original percentages, visible in the column (II), are all much larger than those of the same column in Table II for Schubert. In this Indian music, in other words, intervals descend far more frequently than they ascend, as in fact a cursory glance at the songs will show. And yet approximately the same ratio of decrease from one interval to the next larger emerges as was found most suitable for the analysis of Schubert's songs, viz. 82 %. But a simple decrease of – 18 % per step of a semitone fits the results a good deal better, for there are of course two steps from the fourth to the fifth. It would lead us to negative values for the intervals beyond the fifth, which are impossible. The frequencies beyond the fifth are of course small and will probably express little more than the changes given in the table of mere frequencies (No. 4).

TABLE VI. *Ascent and Descent.*

Percentual frequencies of descent relatively to ascent (=100 %) for the different intervals. Expressed in percentage of that of the major second (II)=100 %.

Nos. of intervals used in songs	2	II	(II)	3	III	4	5	5+	
2 (12)	—	100	(270)	79·3	49·3	37·0	—	—	
3 (57)	81·6	100	(245)	90·9	66·5	33·6	34·0	—	
4 (47)	54·3	100	(278)	74·3	56·5	43·8	7·2	0	
5 (47)	78·1	100	(265)	83·8	84·1	31·7	10·4	0	
6 (13)	47·7	100	(266)	86·4	56·4	72·5	8·3	19·7	
7 (3)	42·1	100	(475)	84·2	84·2	84·2	0	0	
8–10 (4)	45·1	100	(277)	77·3	45·1	45·1	25·8	9·8	
All (183)	63·0	100	(265)	82·6	66·0	41·1	10·9	5·0	
Absol. freq. of ascent	(67)	—	(387)	(399)	(179)	(140)	(83)	(38)	
Densmore (340)	—	100	(276)[1]	82·6	65·9	39·3	10·9	“*sixth*” 25·5	“*seventh*” 22·2
Sioux I Old (147)	65·2	100	(253)	75·9	65·2	49·8	7·4	“*minor 6th*” 8·6	“*major 6th*” 4·4
Sioux II New (93)	56·8	100	(257)	79·4	42·0	47·5	5·6	5·5	—
All the Indian (580)	63·9	100	(249)[2]	85·9	62·6	46·2	9·9	8·9	8·1
Step ratio of 82 %	122	100	—	82	67·2	55·1	37·1	30·4	24·9
Simple Step of –18 %	118	100	—	82	64	46	10	—	—

Agreement between the two types of music extends also to the use of the minor second in ascent and descent. The Indian average varies from 63 to 65 % of the relative frequency of the major second with the lower percentage of 56·8 in the newer Sioux songs and percentages of about 45 in the songs of my analysis that used from 6 to 10 different intervals. The percentage for the figures of Table I (Schubert) varies from 63 for the major passages to 67·6 for all the songs of my analysis and to 72 % for the minor passages. A general tendency of usage in respect of ascent and descent surely underlies both these stages of music, Indian and European.

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[1] The percentual relation of descent to ascent resulting from Miss Densmore's data for this major second is 245. But it is probably really larger, for her frequencies include some 392 minor seconds (cf. Table IV, note 2), for which interval the percentage of descent to ascent is about 100 lower than 265, or 63·0 to 100 as the line "All" of this table shows. If 82·6 is taken over as the reduced percentage for the minor third from this line "All," then the bracketed percentage of descent to ascent for these 340 songs would rise to 276, which is entered in the table for sake of symmetry.

[2] For similar reasons this figure should be rather higher and the percentages in the same line to the right rather lower. But the defect is probably slight. Assuming the percentage for the minor second for my analysis (All), one would have to deduct 5, 4, 3 and 2·5 % from the percentages to the right of the figures (249), a trivial difference.

Similar and contrary motion. The results of my analysis are alone available. They are shown in Table VII. Increase in the size of the interval is evidently accompanied by a rapid increase in the relative frequency of contrary motion, both in respect of the preceding and of the following intervals. The process of increase is much more regular than it was in Schubert's songs. The two kinds of music agree also in showing a greater frequency of contrary motion for the minor second than the direction of increase for the other intervals warrants.

TABLE VII. *Chippewa Music.* (183 *songs.*)

Percentual frequencies of contrary motion relatively to similar motion (=100 %), expressed in percentages of that of the major second (II)=100 %.

In respect of all intervals	2	II	(II)	3	III	4	5	5+
Preceding	112	100	(100)	122	149	230	661	250
Following	131	100	(87·5)	161	215	243	232	1029

The percentages of Table VII, of course, represent the relative frequency of similar and contrary motion for any given interval in respect of all preceding or following intervals, no matter whether large or small, *i.e.* whether the sequence is II–2 or II–II or II–3 or II–5 and so on. The percentage expressions for these single sequences differ enormously from one another, as a moment's consideration of the juxtaposition of intervals in any scale will explain. To illustrate these differences the data for the intervals that follow the major second in Chippewa and in Schubert's songs are given in Table VIII. Nevertheless when these greatly discrepant frequencies are gathered together, the gradual increase in the percentages becomes apparent. We must therefore assume that the influence of the size of the interval interpenetrates all these particular differences.

TABLE VIII. *The frequencies of contrary motion/similar motion for the different intervals that follow the major second in Schubert and in Chippewa.*

In brackets the percentage of contrary to similar (=100).

	2	II	3	III	4	5	5+
Schubert	$\frac{22}{485}$ (4·5)	$\frac{328}{430}$ (76·3)	$\frac{64}{124}$ (51·6)	$\frac{44}{0}$	$\frac{21}{10}$ (210)	$\frac{29}{15}$ (193)	$\frac{20}{3}$ (666)
Chippewa	$\frac{1}{36}$ (2·8)	$\frac{492}{251}$ (196)	$\frac{23}{377}$ (6·1)	$\frac{44}{9}$ (489)	$\frac{4}{4}$ (100)	$\frac{19}{1}$ (1900)	$\frac{5}{0}$

SUMMARY AND CONCLUSIONS.

1. Statistical analysis shows that the frequency with which any interval is used in the songs of Schubert and of the Chippewa and Teton Sioux Indians of North America decreases as the interval increases in size. In Schubert's songs this decrease appears much more regular when minor passages are eliminated from the statistics and when the latter are corrected for the frequency with which each interval occurs in the major scale. In the Indian music the minor third occurs in excessive frequency. Apart from these and other minor discrepancies the ratio of decrease of frequency for each increase of interval of a semitone is approximately 60 % (cf. Tables I and IV and Fig. 1). A similar decrease of frequency attends the use of each interval in Chippewa songs when the interval is only reckoned once for each song in which it occurs (Table V).

2. In the songs of Schubert and of the Indians an interval is used more frequently for descent, relatively to its use for ascent, the smaller it is—except the minor second. Schubert uses the fifth and octave, and still more the tritones, more often for descent than this rule implies. The ratio of decrease of frequency for each step of increase of interval beyond the major second is 82 % approximately in the major passages of Schubert's songs and − 18 % from the major second to the fifth in the Indian songs (Tables II and VI).

3. Similar statistics for similar and contrary motion show that an interval is used more often by contrary motion the larger it is, except the minor second. This holds both in respect of preceding and of following intervals and more regularly as far as the fifth in Indian songs than in Schubert's (Tables III and VII).

No. 1 does not call for any particular comment. It confirms in further detail the fundamental generalisation quoted from Dr Myers at the beginning of this paper, and it favours such theoretical interpretation as I have already proposed in my *Foundations of Music*, where the melodic cogency of the short intervals[1] is shown to be of penetrating importance for the whole structure of music, at least to the end of the recent classical (or "paraphonic") period preceding the present "modern" developments, or in music which is meant to be melodically clear and intelligible.

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[1] The size of intervals and its reciprocal the size of the volumes of tones appear to account for the degree of distinctiveness of musical successive intervals in a way that is agreeable to the results of this paper and to my theory. v. Sterzinger, *op. cit.* pp. 45 f., 57.

No. 3 doubtless finds its *raison d'être* in the limitations of range of the human voice or of any instrument. Having moved in one direction, the voice is more likely to move or to have moved just previously in the other direction. Its average position will presumably be the centre of its range. The greater its leap in one direction, the greater will be the probability of its moving in the opposite direction.

But for No. 2 a reason is not readily imaginable. The rule seems to imply that ascent, not descent, favours melodic cogency. For the larger intervals occur in ascent and presumably they are then melodically fluent. Perhaps the difference is merely one of frequency of use and is therefore due to some extraneous factor determining entry of the voice more often on a high note, as is obviously the case in the Indian songs. But what is this factor? The rule has not, like the other two, been in any way anticipated by analytic writers on harmony, as far as I am aware. It cannot be a mere consequence of either of the other rules, not even of No. 1. The regularity of decrease with size of interval that characterises it is most remarkable. I am unable to suggest any ground for it at all or to illuminate it by reference to any groups of facts other than those referred to in my *Foundations of Music*, pp. 52 and 131.

Note to Tables I *and* IV. The intervals of Turkish melodies counted by Abraham and v. Hornbostel are few in number: thirds, minor 12, neutral 16, major 6 = 34; fourths, diminished 4, pure 8, augmented 4 = 16; fifths 3; minor sixth 1. On the basis of the cents given for each of these intervals, this grouping of them is quite proper. And using the 60 % ratio with due consideration of the cents-centres of these groups gives the values 34, 15·8, 5·7, and 3·4, which is as good a fit as could be expected or desired.—Figures are available also for Swedish folk-songs (*Studien über d. Schw. Volksmelodien*, by Karl Valentin, Leipzig, 1885) kindly brought to my notice by Mr A. H. Fox Strangways. They are for the respective intervals (actually) 56·3, 100, 36·4, 22·5, or with a few diminished fourths 23·4, 17·4, 0, 3·6, 0·7, 0·5, 0·2, 0, 0·3. The first four of these figures are estimated from V.'s count for 20 songs, the rest are his count for 145 songs. The small count seems a fair sample, for in these 20 songs there are 61 larger intervals, which would make a total in the 145 songs proportionately of 442 against 450 actually counted. Using the probable frequencies of the *major* scale, we may correct these frequencies to:

140·7, 100, 45·5, 37·5 or 39·0, 14·5, —, 3·0, 1·2, 0·6, 0·2, —, 0·7.
(165, 100, 47·1, 35·5, 23·6, —, 6·4, 1·0, 0·3, 0·1, —, 0·8.)

(Schubert's corrected major frequencies are set for comparison in the lower line.) Valentin, however, says that *minor* songs greatly predominate. The similarity to Schubert is great: only the fourths and fifths are too few. V.'s figures for ascent and descent are not like any of those above. They are respectively: 89·3, 100, 178, 107 or 110, 46·5, —, 87·0, 51·9, 172, 312, —, 51·9. These agree with Schubert closely enough for the II. 4, 5, 6, and octave. The data given by Rietsch (*Die deutsche Leidweise*, pp. 102, 111) seem similar, but are too meagre and summary to be tested precisely. The matter of this paper is further discussed in an article on "Melody," in *Music and Letters*, 1924, vol. v, no. 3.

EXPERIMENTS ON ANIMAL SPACE PERCEPTION[1]

BY GÉZA RÉVÉSZ.

I. WHY DO HENS NOT PECK IN THE DARK?

II. INVESTIGATIONS OF ILLUSORY SPATIAL PERCEPTION IN HENS.

I. 1.

IT is well known that fowls in searching for their food and in the actual pecking are led exclusively by optical stimuli. When the illumination sinks below a certain definite degree of intensity fowls discontinue to peck even when they are standing in the middle of their grain. It is hardly possible to demonstrate more instructively the particular importance of the visual sense of these animals than by gradually darkening the space in which a hungry fowl is diligently pecking at its grain. The animal then suddenly stops pecking, remains erect for a while, soon sits down and after some time actually falls asleep. It makes no difference whether its feet are touching the grain or not. In the dark one may push or press the birds, throw food in front of them, they react in a way hardly noticeable[2]. They are so dependent on light, that even the rustle of the falling grain, which in the light always excites them, leaves them unresponsive in darkness.

The rustle of the grain that is strewn in front of them may arouse their attention, but this stimulus is not sufficient to cause a peck reaction. This reaction only follows when the food is at the same time seen[3]. This peculiar behaviour of the bird may often be turned to good advantage during experiments. If, for instance, it becomes necessary to leave the room, in which the experiment takes place, for a short while, it is sufficient simply to exclude the light and on returning one may be certain to find the animal on the same spot where it was before. May it be assumed that the short time which is necessary to darken a room is sufficiently long for a hen to forget the grain lying in front of it? The results of experiments on memory of fowls seem to contradict this. It is astounding to notice how long fowls can continue to behave in the way they have been trained. Only a short while ago I was in a position to notice that a hen, which had been trained always to choose the largest from among several large figures, after an interruption of some two months still selected the correct figure in 90 per cent. of the cases. Here, however, the functioning of the memory differs essentially from the case that is being reported.

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[1] A paper read at the 7th International Congress of Psychology, Oxford, July, 1923.

[2] D. Katz and G. Révész, "Experimentelle Studien zur vergleichende Psychologie," *Zeitschr. für angew. Psychol.* 1921, XVIII. 307 ff.

[3] Aimless, partly imitative peck movements apparently occur only in early youth in the period in which movements of biological importance are learnt.

When, as in these cases, an animal reacts upon a certain known stimulus (or upon reappearance of a certain situation with which it was previously acquainted) or abstains from reacting, the stimulus is always *an optical impression actually present,* which in the one case results in the carrying out of the movement, in the other in its inhibition. By training a certain definite reaction becomes firmly associated with a definite stimulus, consequently the performance or the inhibition of the movement takes place fairly automatically—probably without assistance of memory images. Quite different would be the functioning of the memory when in the dark the animal again pecked at its food. It should then be able to conceive that the grain, which it noticed just previously, is still there, and to remember exactly the situation and the spot where grain is lying. When one observes fowls under natural conditions, one does not obtain the impression that these animals could have clear memory images of grain that has become invisible.

I performed experiments with fowls in which I dug in grain or covered it with sand under their very eyes. The animals made a few inexact, mostly unpurposive scratch movements, undoubtedly preponderantly in the vicinity where the grain was hidden, nevertheless they soon ceased scratching, no matter whether they had found some food or not, and ran away without looking backwards. Their scratching appears to me to be an instinct, which is not more purposively developed by individual experience. Often I only covered the wheat with a piece of paper, the result was the same: the necessary movements with their beaks or feet were not performed. When by scratching the paper became accidentally displaced the fowl naturally picked up the grain at once. When, however, it did not succeed in finding the grain at once, it did not exert itself any further. Whenever some corn became visible under the paper the fowls endeavoured to push back the cover in order to obtain the partly hidden grain. In any case, therefore, an optical stimulus must be present to induce the animals to peck movements.

The monkey also is in this respect a visual animal, guided preferably by its visual sense in all its movements. And yet how great a difference between these two! When a monkey can see me placing a tit-bit in a small box or wrapping it up in a piece of paper, it will endeavour to obtain the food and will continue its efforts till it has achieved its object, till its attention is diverted by something else, or till it has become 'convinced' of the fruitlessness of its labours. Not infrequently it returns to its task and continues its interrupted work with equal ardour[1].

The monkey can, like a fowl, rely on its visual impressions and presentations but over and above these on its *intelligence* and *experiences*. It is well known that sensations of smell and even those of taste and touch are of no importance to the fowl and the monkey alike; at most these sensations play only a subsidiary part. Buytendyk and Révész showed this with various species of monkeys[2]. It was proved that they could perceive no difference between a real orange and an excellent imitation. When from a distance of 10–30 cms. a real orange and one made of *papier mâché* were handed to the animals, they behaved in each case in *exactly the same way*. Already at the mere sight of the real fruit as well as of the imitation they become extraordinary excited, stretched out their hands, did not turn away for a moment from the coveted fruit, emitted peculiar cries of excitement and impatience, jumped about aimlessly and came back at once; they attacked their comrades whenever these ventured near the grating. The entire absence of any scent in the case of the imitation orange did not appear to make any noticeable impression on them. One could touch their stretched-out fingers with the paper model, but even then their behaviour remained unaltered. How inferior the other sense impressions are in comparison with the optical sensations is clearly shown when the monkeys were given the parts of the imitated orange, made of *papier mâché* and plaster of Paris. Undoubtedly the parts of the rind had been well imitated, the concave side was orange-coloured with the characteristic roughness, while the convex side looked like the real white substance. Although the broken-off parts felt quite *different to touch* from those of a real orange (daily the animals enjoy the real fruit in abundance on account of the generosity of visiting children), the monkeys rely so much on their visual impressions that they do not entrust their other senses with even an informative part, still less a *corrective* one. Their visual sensations count for so much that they would not be convinced by repeated trials when these contradicted their optical experiences. A mandril, *e.g.*, put a piece of imitation rind into its mouth; becoming attentive by the unusual taste, it took it out again, looked at it carefully, smelled at it, put it into its mouth again, took it out again, once more it smelled at it, and after these protracted examinations at last it threw the piece away.

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[1] The difference between a fowl and a dog is apparently not so great; for the dog is guided like a fowl by a stimulus actually present—in this case its olfactory sense. Still a well-bred dog appears to be able to seek hidden things even when the object cannot be detected by its smell. It would only be necessary for the dog to have seen where the object was hidden. The possibility that here certain odours only discernible by a dog are assisting cannot be denied.

[2] I. J. J. Buytendyk et G. Révész, "L'importance spéciale du sens de la vue dans les phénomènes de reconnaissance chez les singes inférieurs," *Archives néerlandaises de Physiologie*, 1923, VIII. 14 ff.

When, therefore, a visual animal like the monkey is searching for hidden objects it must undoubtedly be governed somehow or other by the representation of the object. Without this supposition it would be impossible to explain the many and varied methods and ways the monkey tries.

Contrary to this the fowl, although a visual animal also, does not search for hidden objects under normal conditions. Pecking at the paper cover, as mentioned previously, may perhaps be looked upon *as the immediate after-effect of optical impressions just disappeared* (after-sensation). The food instinct induces the fowl to run about to examine its vicinity, to scratch, but does not keep it to a spot where before its eyes food is being hidden. Had the animal retained a representation of the food that had lain in front of it, it would not have left so disinterestedly the spot where the grain was hidden. It is, however, possible that one could develop in these animals the habit of coming to a particular spot when hungry, but this is merely a training, which does not now concern us.

The behaviour of the fowl toward objects hidden in its presence shows that it either does not possess any visual representations at all, or if it has such images they are so confused and indefinite that they cannot be fixed and are not to be considered as *motives to actions*. Taking for granted, however, that the fowl possesses fairly distinct representations, it must be denied that function by which these representations can be retained for a considerable time in spite of inhibiting and opposing impressions and influences. Images emerging immediately after primary perceptions are dragged along by the flood of numerous fresh impressions. I suppose these circumstances to be, to some extent, the reason why the animal is incapable of benefiting by *one simple experience*, why it can hardly ever be taught by going through an experience only once. The hen, therefore, always requires an accumulation of single experiences (*i.e.* training) in order to come to an efficient experience. By virtue of the plasticity of its disposition it would be possible by training to induce the fowl to push away with its beak a paper which covers its food[1].

In this case I proceeded as follows: at first the corn was covered with a *transparent paper* which was pierced at once by very forcible pecking at the spots where the single grains were lying. When next I covered the corn with an *opaque* paper the animal did not peck at it. In some of the first experiments it happened once or twice that the fowl pecked at the opaque paper but soon stopped. Even in those instances, in which I lifted the covering paper, in order to draw the attention of the animal to the wheat—a movement which always induced it to a peck action—it did not attack the paper.

The method was now altered. While the animal was pecking at corn I covered up a *portion* of the food. This action did not have the slightest effect on the fowl (a monkey or a dog would have snatched *at the hidden food first*), it continued its feed till all the visible corn was eaten, then it raised itself quietly and did not make the slightest effort to get at the hidden corn. Here and there it pecked at the paper hesitatingly, but soon made no further effort. Already at the third repetition a change in its behaviour could be detected. The animal pecked at the covering paper five times in succession. Gradually it pushed the cover farther and farther, till at last after the sixth repetition the animal quietly pushed the paper aside. When I used hard cardboard instead of paper to cover the wheat the animal belaboured the hard cover with its beak in the manner of previous experiences, but being unable to push it aside as in the case of the paper cover it soon stopped all efforts. When the experiment was repeated with a variety of covering sheets, the bird gradually grasped the situation and at last endeavoured to push aside everything that was lying in its vicinity. The experiment shows, therefore, that although under ordinary circumstances the animal *does not search for hidden grains*, it can easily be trained to do so. This training must be arranged in such a way that the animal is kept actively engaged by optical stimuli.

Still it is easy to create a situation in which the animal will not be able to find its bearings notwithstanding the stimulation of optical sensations, or more correctly *because of optical stimuli*. When, *e.g.*, I cover the wheat with a *pane of glass*, the fowl cannot be induced to push it away, no matter what methods one chooses to employ. The stimulus exerted by the grain is so compelling that for quite a long time it cannot desist from pecking at the plate of glass and when at last it realises that the corn visible under the glass cannot be picked up, it indeed stops pecking but does not push aside the pane of glass like the other covers. All imaginable endeavours to train the animal to remove the glass plate were in vain. The animal cannot conceive *that a grain can be visible and still not attainable.* Gradually it learns to distinguish the corn on the glass plate from that lying under it, but it never conceives that the pane of glass is a cover like every other, hence it never endeavours to push it aside. The phenomenon of *transparency* certainly falls outside the physical experiences of our hens.

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[1] The animals for the purpose of the experiments were placed at my disposal by the Psychological Laboratory of the Psychiatric-Neurological Clinic at Amsterdam, for which I beg to tender my appreciation to Prof. Dr K. H. Bouman.

I believe that the search for food and the feeding itself can only be fully understood after we have subjected the elements of *the pecking process* to a close scrutiny, which has hitherto been neglected.

2.

The hen seizes its food by pecking at it. In a short time it develops great accuracy of aim. Elsewhere I have stated that this ability is so well developed that *e.g.* the hen pecked at crushed grains of rice of about 0·5 mm. length with so great an accuracy that it did not miss once[1].

What are the composing elements of an act of picking? In reality it consists of a sensori-motor adjustment to an aim, *i.e. aiming,* followed by the execution of a quick *movement of the head* and concluded by the *picking up of the grain* by means of the beak. What may now be concluded? Obviously that the animal can only peck at an object after it has *aimed at it.* One can only aim at visible objects; it is clear, therefore, why the hen never adjusts itself to invisible (hidden) aims, and very rarely to objects that are indistinct. The hen is by nature limited to this sole manner of seizing its food: that is the reason why it stops picking as soon as the aim disappears. The hen cannot search for, feel or grip with its beak, it can only peck. Hence it follows that whenever the actual conditions which make aiming possible are failing, the animal is *nolens volens* compelled to deny itself all nourishment.

Not only an aim is necessary, but also a certain *position of the head* in relation to the grain. I designate the distance between the beak and the grain, which must be observed in order to be able to peck, as the *picking elevation.* When one places the beak of the hen in a basin filled with corn it does not eat, because it cannot do so; it only begins when its head can take up a position at a certain distance from the grain, *i.e.* at the picking elevation. This fact may also be observed when the animal is fed by hand.

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[1] G. Révész, "Tierpsychologische Untersuchungen," *Zeitschr. f. Psychol.* 1921, LXXXVIII. 130 ff.

The picking elevation varies between ½–3 cms. Aiming at the grain can take place from a greater distance; the hen usually aims at an object at a distance of about 7–9 cms. (aiming elevation). Whenever an animal has consumed its corn, it does not at once take up the natural position of the body nor does it immediately poise its head at the usual height, which amounts to about 22–28 cms. in medium-sized fowls; but it lifts up its head *jerkingly*, at first to approximately the aiming elevation and then it reaches the natural position of its head by one more transitional stage. Although these three stages are passed through fairly quickly, yet they can be sharply distinguished. It appears to me that from the first stage the animal surveys its area of vision, it reconnoitres the field. Hence it may be called the *attention position*. Often it happens that the hen pecks directly from this attention position.

The bird endeavours to retain the picking elevation *invariably*. Further, it strives to maintain the eye in a position which will enable it to obtain a full view of the field extended in front. It is interesting to notice the manner in which the bird adapts itself to momentarily altered circumstances in order to restore conditions most favourable for aiming and striking.

When one places wheat on a support 25 cms. high, the bird stretches itself so as to place its head at a height of about 34 cms. In this way it succeeds in retaining the picking elevation approximately unaltered. The jerking movement mentioned previously is now reduced to two stages, *i.e.* the attention position and the elevation of the head (34 cms.), which is altered by raising the level at which the grain is presented. If this level be further raised to 31 cms. the elevation of the head becomes 37 cms. Picking elevation and attention position (6–7 cms.) remain unchanged. By further increasing the height of the support to 38 cms. we compel the bird to raise its head to a height of 44 cms., *i.e.* it keeps its head at an elevation 6 cms. above the support. The picking elevation becomes somewhat less, but it never lies below the lower limit of ½–1 cm. The pick movement is always followed by a jerk upwards, followed by one backwards. When the bird is not feeding it returns its head to the normal height of 34–35 cms. Even with the support at 31 cms. from the ground the picking elevation shows itself normal, but at 44 cms. this cannot be ascertained and the two jerking movements following are now both downwards. If the grain be placed at a still greater height, the bird, which in the meantime has assumed giraffe-like proportions, does not react.

Gathering together the threads of the argument we may say *that as the hen only pecks at objects at which it can aim optically, it fails in darkness.*

3.

We have elsewhere ventured to utter the presumption, that a hen placed in the midst of a quantity of excellent food would die of hunger, if it could not see this nourishment[1]. In order to solve the problem I have placed a hen in a large cage, which was now made lightproof by a cover, and for seven hours the animal was left to its meditations. After opening the cage *the* 90 *grains were all found lying on the bottom.* After a few days I repeated the experiment but now prolonged the imprisonment to 24 hours. Thirty-five large maize seeds were placed in the box so that the attention of the animal would be attracted while strutting about. On opening the box we found the 35 seeds intact. When the bird was liberated it made an onslaught on the grains and picked them up in rapid succession (18 seconds). In the third experiment the hen was kept 39 hours in the box with the same result.

A control experiment with another bird had the same conclusion. This bird was taken from the fowl-run on the 7th February at 5 o'clock in the afternoon and placed in the box together *with a China basin* containing 350 maize and wheat grains. On the 10th February at 10 o'clock a.m. the box was opened and the full quantity, 350 grains, were found partly in the dish, which was upset, and partly on the floor. The animal, therefore, had been confined to the box for almost three full days without touching a single grain. Realising the usual gluttony of fowls, one may, on the one hand, arrive at the biological significance of their visual sense, on the other hand at a true estimation of their adaptability and initiative.

After some months I have repeated these experiments with several hens in the dark box. It appeared that with the exception of one bird—undoubtedly an animal extraordinary quick in acquiring habits—which at the fourth repetition of the experiment had seized half the number of grains (60) during its stay of 2½ days in the dark room, the remaining five hens had during a first period of 48 hours and later during a subsequent detention of 68 hours not *managed to seize a single grain.* It is then evidently an exception when hens—without previous training—search for and seize grain when confined to darkness.

In his excellent study on "Development of Certain Instincts and Habits in Chicks," F. S. Breed discusses my views on the behaviour of hens in the dark[1]. He maintains that "the assumption that chicks do not eat when they cannot see surely is not borne out by observations of the behaviour of chicks in the dark room." As this author attached some weight to the solution of the problem, he conducted some further experiments, in which he came to the conclusion—contrary to our observations—that hens when placed in absolute darkness begin to pick at their food, provided that the animals were allowed to feed out of the basket *before* light was excluded; in other words, when the hens were previously adjusted to pecking movements. "The pecking sounds, as the bills of the chicks struck the bottom of the dark box, could be distinctly heard."

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[1] D. Katz and G. Révész, *op. cit.* 315 ff.

Breed's statement has indeed surprised me considerably, as on the one hand his data could not be doubted, but on the other I had been so often in a position to observe how hens placed in darkness cease pecking at once. I must admit that I cannot explain Breed's results, more especially because I am ignorant of essential details, *e.g.* I do not know whether all hens seized the grains or perhaps only a single bird, while the others solely picked in an imitative way without actually seizing the corn. Obviously the experimenter could not explain this satisfactorily, because he could not see the animals. Further, it is not stated whether the grains were *counted before* the exclusion of light as also after the conclusion of the experiment, for it is quite possible that the hens being adjusted to the pecking movements continued this action aimlessly without *actually seizing* any food. I am fully aware that certain individual animals continue to pick a few times in darkness, but also that they seldom succeed in seizing something and soon become inactive.

These considerations have induced me to repeat the experiments described by Breed. My observations are as follows: when light is excluded *suddenly and absolutely* when the hens are feeding they *cease feeding at once.* When the space is again illuminated they commence to pick again; the moment darkness ensues the birds stop to feed; and this demonstration may be repeated *ad libitum.* It is immaterial whether this experiment is performed with one or more hens, in the latter case the phenomenon is even more demonstrative. Undoubtedly it happens occasionally that some animals continue to pick after the light has been suddenly switched off, but this is exceptional and these efforts are futile. The hens merely strike but do not manage to seize any grains. These observations, moreover, could be recorded with only two animals, one of which in one instance continued to pick twice, scoring one hit, at another time it struck eight times seizing nothing. The other birds discontinued to feed at once, and did not move from the spot. We may, therefore, conclude that as a rule hens in absolute darkness *do not pick*; in cases, however, where they continue to execute some pecking movements owing to previous motor adjustment, they abstain from making further efforts as soon as they have picked a few times unsuccessfully.

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[1] F. S. Breed, "The Development of certain Instincts and Habits in Chicks," *Behaviour Monographs*, 1911, I. No. 1.

Totally different results are obtained when the *illumination is gradually diminished*. In this case it is evident that there is a *critical*—individually determined—*degree of intensity of illumination* at which hens desist from picking. Indeed the animals can still see the grains in this twilight and now and again they execute some fairly accurately localised picking actions, yet without seizing the grains. Often they do not go beyond *incomplete aiming movements*, viz. abrupt movements of the head, which at the last moment are inhibited at a distance of about 0·5 cm. from the plate[1]. At first one bird made about 3–4 such incomplete pecking movements to one completed reaction at this critical degree of lighting intensity. Later on this improved to 30 incomplete reactions out of a total of 60 movements. With another animal I counted 36 inhibited reactions out of a total of 62 reactions and with a third hen this rose to 62 out of 87. These rapid little movements may be assumed to be *attempts at aiming* at indistinctly seen objects, which assumption gains strength by the fact that these movements *cease entirely* when the *illumination is lowered* still further. This, however, is not so in the case of the *actual* pecking reactions that are purely motorically conditioned. Among seven hens I have only been able to find three that showed these inhibited pecking reactions. When the others reacted at all they always pecked at the plate.

On gradually diminishing the lighting intensity, the pecking reactions are retarded and the number of false reactions is steadily increased. Under normal lighting conditions the animals go through 30–38 seizing reactions in 15 seconds, but in twilight it usually takes some minutes for the same number of reactions. One may form an estimation of the uncertainty of aim under twilight conditions when it is considered that the animals placed in light of 'critical' intensity can rarely seize more than 10–14 grains out of 50 in the course of 40–50 pecking reactions. When it is further mentioned that in these experiments all grains were placed in a heap on a shining black plate in front of the animal; it is easy to imagine what little chance the birds can have to peck at grains lying scattered in a dark box.

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[1] The different stages in the pecking reaction are indicated by C. L. Morgan (*Habit and Instinct*, London, 1896) by the terms: *striking*, *seizing* and *swallowing*. To these three stages, which are also adopted by Breed (p. 15), should be added as a fourth the above-mentioned *incomplete* (unfinished) *pecking*, *i.e.* aiming followed by inhibited striking.

In pecking reactions performed in twilight, the aiming and striking elevations approximately coincide; these hens strike from a distance of 2–3 cms. which is somewhat less than the usual striking elevation and still less than the aiming elevation. It appeared to me that by altering the intensity of the light we procure a gradual lowering of aiming and striking elevations.

The experiments described above also support my contention *that hens cannot only strike after aiming.* That is the reason why they do not strike in darkness, but are able to do so to some extent in twilight. This also explains why the critical intensity of the illumination is subject to individual difference (individual adaptation, certainty of aim) and why the pecking reactions are gradually retarded upon lowering the intensity of the light, and the number of correct reactions (seizing) grows smaller. Hence also the previously mentioned incompletely executed aiming and striking reactions, which only appear when the objects to be aimed at become indistinct and which is soon followed by cessation of all efforts on the part of the animals when this uncertainty of aim leads to no results. Nevertheless there is possibility of inducing hens to react even in absolute darkness. For this purpose one should hold the feeding basin well filled with food in front of the animals in ample light, and allow them to feed quietly for some time; suddenly the light should be switched off. It may now be observed that several hens continue to peck. When they stop—which usually happens very soon—it is possible now and again to incite the birds to further reaction by shaking the grain. This, however, is not the case with all hens; some refuse to react even when their heads are pushed into the basin, while others, on the contrary, are ever trying to find the dish again, when they have lost it. It must be observed that under these conditions the hens *no longer take aim,* but simply peck away at the contents of the receptacle.

This, moreover, is the method that should always be adopted when one desires *to train hens, and all other animals that pick or seize their food after aiming at it, to feed regularly in darkness.* I succeeded, *e.g.* in this manner, in training three hens, that previously *had failed to seize a single grain* after remaining 68 hours in darkness, *to empty a basin full of food* after only 20 hours in darkness. In a similar way blind birds may be taught to feed themselves[1].

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[1] Animals born blind or becoming blind usually perish. In captivity, however, it is possible to teach them to feed themselves, *e.g.* by restricting their freedom of movement so

The experiment with the feeding basin was also repeated which led to the above-mentioned results. When the birds, which were by now somewhat accustomed to darkness, were again placed in the dark box on *the bottom* of which grain was strewn (none in the basin) they did not manage after 20 hours to seize this food (out of 150 grains only 10 were missing). But whenever I placed in the dark box two basins that could be easily upset filled with corn instead of a firm one, they continued to feed from these receptacles until these were upset. They did not, however, peck at the grains that had fallen out, or to be quite accurate one could hear them peck at the bottom of the box for a time but they soon stopped. A similar result is obtained by strewing the bottom of the box around the filled basins fairly thickly with grain. The birds prefer to feed from these basins but now and again they pecked at the floor. Yet after a few minutes they stopped[1].

As long as they are incited by the memory images of the basins and the grain strewn on the floor and by their successful efforts in the dark they continue their pecking reactions, but as soon as the number of their failures increases they gradually discontinue their *blind* reactions. Among my birds I have only discovered two hens that in the dark continued to search for grains for some considerable time; at the same time I have never once noticed these two animals when standing in the middle of their grain to cease pecking immediately upon the room being darkened.

Needless to say, these observations confirm my views on the nature of the pecking reaction which is always preceded by aiming.

4.

In order to obtain a clear conception of the picking mechanism and concomitant behaviour, I should like to quote an analogous instance of human conduct. When we wish to catch a fly, we adjust ourselves sensori-motorily by closely watching our prey, by focusing our eyes on it, by preparing ourselves for a definite movement in order to make a rapid and precise attack from a distance determined by experience. By analyzing this process it is possible to detect all the essential factors which we discovered in the act of picking: *attentive observation* from a considerable distance (*attention elevation*); *focusing* the eyes on the object (*aiming*), *neuro-motor adjustment* in preparation of the impending move-

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that the animals can only move in front of their feeding basin and thus must come in contact with it continually. In a similar manner it should be possible to induce hens in darkness to take food. Portielje in *Ardea*, Amsterdam, 1922, deals with unnatural feeding methods learnt by animals. See further Breed, *op. cit.* 25.

[1] So far as I could determine the picking elevation in the dark was from 2–3 cms.

ment; *rapid execution* of the movement from a certain determined distance. If we miss the fly entirely, or if our movement is partly inaccurate, the insect escapes; similarly the grain of corn is shot away when pecked at inaccurately. As we can only catch the fly by a particular movement and by that movement only, so the hen can obtain its wheat exclusively in one way by picking at it. We can only catch a fly at rest or in slow motion, and the hen suffers the same limitation with its grain. My readers may perhaps be so resourceful as to find a better and a more illuminative example.

The behaviour of the hen loses much of its remarkableness but, on the other hand, regains a considerable portion of its prestige, so unmercifully attacked by us, when we illustrate the bird's situation by an example.

Let it be imagined that we were confined to an absolutely dark space in which the presence of food could not be presumed. Only in a condition of ravenous hunger, of intense despair, would we search for food. But I can hardly believe that even in this case a systematic search of the space would be resorted to. We should now be in a condition similar to that of our hen which would in voracious hunger perhaps pick about a few times and accidentally not finding anything would stop searching. Naturally we should not till then have postponed the investigation of our prison; we should have adapted our methods guided by insight and deliberation. But it is just on account of this that we are such wonderful creatures who can, while struggling along in their search for food, wax enthusiastic about the psychology of hens.

II.

INVESTIGATIONS OF ILLUSORY SPATIAL PERCEPTION IN HENS.

1.

The problem to be solved by these investigations is to determine whether animals are subject to the same optical illusions as men are. This question has interested us in two ways. Firstly, we desired to know whether optical phenomena, which cannot be explained *without the collaboration of central psychological factors*, could also be discovered in animals. Secondly, we wished to ascertain whether it was possible to find a method which would lead to absolutely reliable results with animals.

It has often been attempted to explain optical illusions without reference to psychological factors; up to the present this endeavour has only in a very limited number of cases led to a somewhat satisfactory result. On the other hand, it became manifest that such psychological motives as attention, the different modes of perceiving form, adjustment to the whole or to its elements, the absence of clues for judging objective proportions and other psychological motives may evidently affect the illusions.

The optical illusions of size belong to the type of optical phenomena which do not permit of a *purely physiological* explanation. The impressions in the case of these illusions are not determined unequivocably by the corresponding stimuli. Size, often even space perception (flatness or perspective form) depends on *psychological factors*. The parallel lines in Zöllner's diagram, which appear to converge or diverge according to the position of the little slanting cross lines, can by no manner of psychological adjustment be seen as actually parallel. Even if it be granted that at a closer comparison of opposing points of intersection the illusion appears less, apparent parallelism is not attained and as soon as the attention is directed to the whole figure the illusion reappears in its original distinctness. Viewing the elements of other optical illusions in isolation causes little or no change, as *e.g.* in the Hering's star-figure or in the well-known pseudoscopic radiation figure. The direction of the attention and the intention does not appear to have any influence here.

Similar phenomena appear when we place two congruent trapezia one above the other. The trapezium above appears to be larger than the one below. We remain subject to this illusion even when we are convinced that the figures are congruent. It struck me, however, that one only becomes conscious of the apparent unequality of the trapezia by directing the attention on both figures simultaneously. We do not learn of their mutual relations by casually glancing at the figures, and it is precisely the perception of this relation that is one of the most important causes of the effectiveness of the illusion. I have the impression—together with many of my observers—that the illusion only becomes perfect when the attention is intensely concentrated; the *development of the illusion* can be directly experienced during attentive observation.

A similar illusion, even more distinct, may be experienced in segments which though actually equal and of exactly the same area yet appear unequal (Jastrow illusion).

The upper segment of congruent figures appears considerably *smaller*. If the two figures be so placed that the lower corners of the upper segment *almost* touch the upper arc of the lower segment, a diminution of the illusion cannot be obtained by concentrating the attention; this, however, is the case when the two figures are farther apart. On comparing both the inner and outer arcs separately and successively, the illusion may disappear entirely for some moments. During this comparison the two figures together may never be perceived as one whole group, for then the illusion would again become distinctly noticeable. (Similarly when both figures are separately and successively compared as units; in this case one must guard against effacing the isolated impressions of the separate figures by unintentionally combining both segments.)

When a large number of such congruent segments are placed in a row one above the other, one gains the impression that there is a difference in size between the *upper* and *lower* segments, while the interjacent segments all appear equal; but upon isolating *any two segments* together, selected indiscriminately from among the upper, lower or intervening figures, the illusion in these two suddenly reappears. These observations clearly show that the illusion only becomes manifest when both figures are simultaneously and collectively perceived. It becomes evident, therefore, that this illusion presupposes a certain definite mode of perception. Whether this simultaneous perception is quite unconscious and unintentional, or whether some intentional factor must be added to the perceptual contents, is a question beyond the scope of this investigation. One may also say that it is evident that without the cooperation of psychic factors this illusion could not be procured.

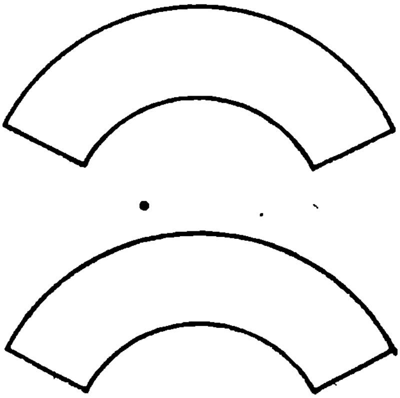

Fig. 1.

2.

After what has just been said on the subject, it should prove interesting to investigate the presence of optical illusions with geometrical figures in creatures of such simple psychic structure as hens.

In the investigation of this problem we proceeded as follows:

At first the bird was trained always to choose the *smaller* of any two figures. Two circles of different areas strewn with wheat were presented to the bird. The hen had to learn to choose the smaller circle and to avoid the larger. Whenever the bird pecked at the prohibited figure it was either drawn away or we frightened back the bird by a quick and unexpected movement of the hand[1]. With this method the training proceeded *much* more slowly than with the gluing method, because the stimulus excited by the larger (prohibited) circle never lost its effectiveness. The forbidden grain could be picked up, which was impossible when the food was glued on to the figure. It could, therefore, not be expected that the animal would arrive at perfectly errorless reaction, consequently we had to be satisfied when the correct reactions exceeded the wrong ones in a large number of cases. When *e.g.* the bird reacted correctly in 20 out of 24 tests (83 per cent.) the training was considered complete. The training in connection with the circles exercised its influence on pairs of other figures that were presented to the animal for the first time. Of squares, parallelograms and triangles, the animal in the majority of cases chose the smaller figure *without any introductory training* (Table I). The bird must have learnt during its training *with circles* to be guided solely by difference in size or by the direction of the 'transition experience' from large to small (*Übergangserlebnis*: Jaensch-Lindworsky).

It is possible in this way to explain how the animal could maintain this previous adjustment to *quite unknown figures*.

On the second day of the investigation I again began the training experiment with the circles and continued with the other pairs of figures. The relative number of correct reactions increased from 81 per cent. to 83 per cent.; on the third day this was further improved to 90 per cent. We now considered the training on the smaller figure completed. In Table I information will be found on the course of the introductory training experiment. The initial experiments with the circles are not included in this table. The column 'Stimulus' indicates the pairs of figures employed.

Before we passed on to the real experiment on illusion with the

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[1] The old method originally introduced by Katz and Révész ("Experimentell-psychologische Untersuchungen an Huhnern," *Zeitschr. f. Psychol.* 1908, L. 83 ff. See further T. B. Watson, *Behavior*, New York, 1914, 68) by which the forbidden grain was glued on the figure we abandoned, because it was too tedious carefully to glue on the grains again and again. This free method of training had the additional advantage of allowing the whole series of experiments to be carried out continually, while the gluing method often compelled us to cease operating suddenly at the crucial test.

congruent segments, we trained the bird for the sake of further practice with two *segments of different areas*. The hen reacted on the *smaller segment* in accordance with its previous training. Attention must now be drawn to a difficulty in which the bird was placed in the subsequent crucial experiment on illusion which was not met with in its previous training. Two identical segments were placed one above the other, and in this experiment—assuming always that the bird was subject to optical illusions of size—the animal should choose the farther figure, *i.e.* the subjectively smaller one. In order to accomplish this it must leave untouched the *subjectively larger* segment immediately in front of it, and *across this figure* it must peck at the grain on the remoter segment. If one can imagine the difficulty involved in this task, it must be conceded that one may not expect this accomplishment invariably.

Table I. *Training experiment on perception of differences in size.*

Date	Series	Stimulus	No. of correct reactions	No. of errors	Correct reactions in %	Errors in %	Remarks
Dec. 13	1	○ ○	43	7	86	14	[*]
	2	▭ ▭	25	8	76	24	
Dec. 14	3	○ ○	20	4	83	17	[†]
	4	□ □	10	3	77	23	[‡]
	5	△ △	9	1	90	10	
	6	○ ○	30	5	80	20	
Dec. 16	7	○ ○	18	2	90	10	
	8	□ □	5	1	83	17	
	9	△ △	6	1	86	14	
	10	▭ ▭	4	0	100	0	

[*] The figures used as stimuli were of the following dimensions: diameter of circles 9·5 cms. and 8 cms.; sides of squares 6·3 cms. and 5 cms.; bases of triangles 7·5 cms. and 6 cms.; altitudes respectively 6·5 cms. and 5·3 cms.; parallelograms 4 × 8·5 cms. and 4 × 7·5 cms. Width of segments 2·6 cms.; arc of the larger circle 120°, of the smaller 80°; arc of the congruent segments equal to the larger one 120°.

[†] The position of the two figures was reversed.

[‡] To erroneous reactions a lower value must be assigned than to correct ones, for it became evident that the animal after committing an error *much more frequently* pecked at the correct figure than happened reversely. When the wheat on the smaller figure was consumed, the hen was wont to stop pecking and to wait patiently for the next experiment. Now and then the bird looked at the larger figure attentively, without touching it. But, whenever it had begun to consume the grain on the larger segment first, it usually continued its feed at once on the smaller figure.

It was our task, therefore, to eliminate this difficulty, and we could only hope to succeed if we could manage to induce the bird to fix its

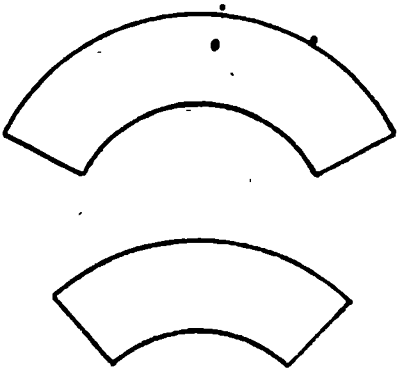


Fig. 2.

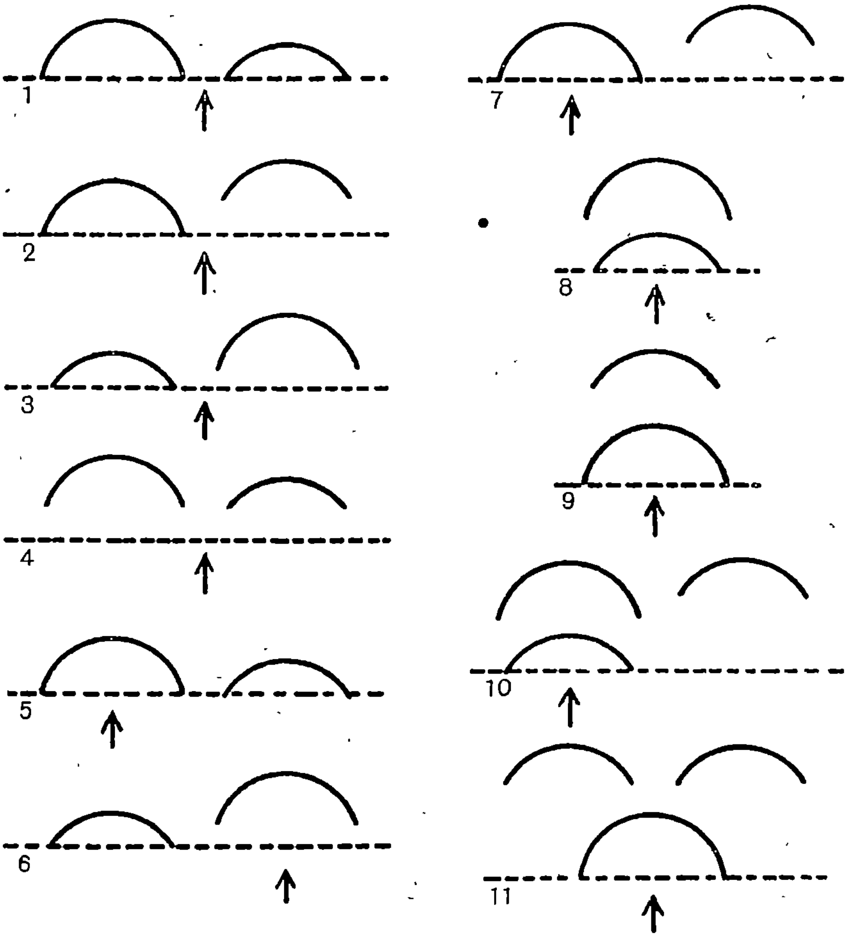


Fig. 3.

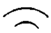
attention so intensely on the smaller figure that alternations in the position of the two figures should exercise little or no influence. The hen, therefore, had next to be trained to select food on a smaller figure that was placed in a more *awkward and remote position* in preference to grain lying closely at hand. In this we succeeded by continually changing the spatial relations between the two unequal figures in a separate series of experiments. In this way the animal became accustomed to single out the smaller figure with particular attention.

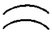
As the above diagram indicates the smaller figure first stood beneath the larger one, *i.e.* nearer to the animal, then above, *i.e.* farther away from the bird. Next it was placed *above to the right or to the left* so that the bird stood either *between both* figures or *immediately in front of the larger segment*. Often we placed both figures *next to each other* with the bird *in front of the larger*, or we pushed both far away from the position of the hen so as to compel it to take a few steps forward—*i.e.* towards the forbidden segment—before the bird could peck at the wheat. Under these difficult circumstances the animal committed only 3 errors in 27 tests for the first time; in the next experiment it showed 5 errors in 26 reactions.

3.

After these initial training experiments we at last passed on to the investigation of the illusion.

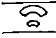
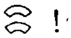
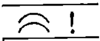
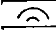
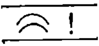
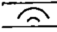
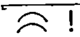
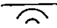
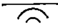
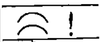
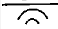
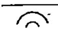
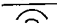
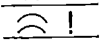
Two congruent segments were placed at such a distance from each other as was particularly favourable to the perception of the illusion (Fig. 1). The hen usually stood ± 10 cms. away from the subjectively larger segment so that the subjectively smaller figure was about 17 cms. from the animal. On both figures we placed two grains of wheat or rice. As soon as the animal was released by us it began to peck at the grains on the *subjectively smaller* segment, while it left those on the subjectively larger figure untouched. On the first day 13 experiments were made with 12 *positive reactions* relative to the illusion. On the second day the hen reacted seven times correctly in nine tests.

For the purpose of demonstrating the entire training and testing process we have tabulated the whole series of experiments of hen *A* (Table II). The changing position of the *unequal* pairs of segments designated  is indicated by numbers in the Remarks column. For an explanation of these numbers see Fig. 2.

The objectively equal segments used in the experiment on illusion are designated .

Controls were provided for by repeating the experiments with the

TABLE II. *Experiment on perception of spatial illusion with congruent segments.*

Date	Series	Stimulus	No. of correct reactions	No. of errors	Correct reactions in %	Errors in %	Remarks
Dec. 13	1	○ ○	65	15	81	19	
	2	▭ ▭					
Dec. 14	3	○ ○	69	13	84	16	
	4	□ □					
	5	△ △					
Dec. 16	6	○ ○	33	4	89	11	
	7	□ □					
	8	△ △					
	9	▭ ▭					
	10		27	3	90	10	Position 1–9
	11		4	1	—	—	
	12		4	0	—	—	„ 1
			4	1	—	—	„ 4 and 8
	13		6	0	—	—	
	14		5	0	—	—	„ 8
			5	1	—	—	„ 9
	15		3	0	—	—	
	16		3	0	—	—	„ 8
Dec. 18	17	○ ○ □ □ △ △	19	1	95	5	
	18	▭ ▭	7	3	70	30	
	19		17	3	85	15	„ 5 and 7
	20		9	1	90	10	„ 1, 5, 6, 8, 9
	21		7	2	77	33	
Feb. 6	22	○ ○	18	13	58	42	
	23	□ □	23	6	79	21	
	24		9	2	82	18	„ 1. Exp. here [interrupted
Feb. 7	25	○ ○	8	0	—	—	
	26	□ □	9	15	38	62	
	27		10	4	71	29	„ 1, 7, 9
	28		4	2	—	—	
Feb. 14	29	○ ○	34	3	90	10	
	30	△ △	17	2	—	—	
	31	□ □	20	5	80	20	
	32		20	4	83	17	„ 1
	33		8	15	35	65	„ 5
	34	▭ ▭	21	6	77	23	Analogous position 9
	35		25	9	73	27	Position 1, 8
	36		7	0	—	—	

objectively different segments as well as with those differing subjectively. In the latter experiments the *position of the bird* in relation to the segments was sometimes changed, or *the distance between the segments* was altered. This latter alteration was limited to a distance of from 5 to 40 mm.

The *most decisive test* to which we used to submit the animal, after each series of experiments was concluded, as regards the principle involved, was as follows:

We confronted the animal with three instead of two segments, of which two were *identical* and one *objectively smaller*. The three figures were placed above each other with the objectively smaller one below, the two identical ones respectively in the centre and above (Fig. 4).

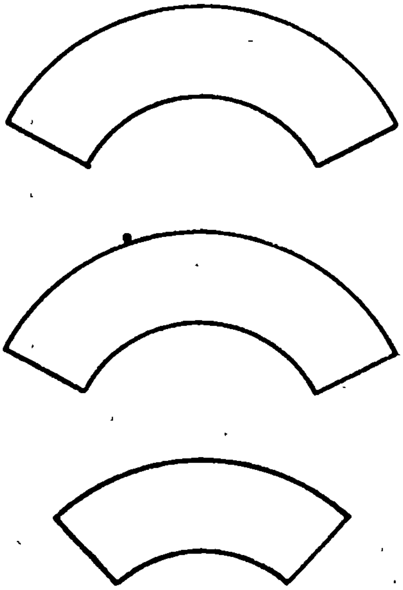


Fig. 4.

To us the segment below, *i.e.* the nearest (objectively smaller), and the one above, *i.e.* farthest away (subjectively smaller), appeared distinctly smaller than the one in the middle. We conjectured that whereas the animal had been trained to react on the smaller figure it was just possible, taking for granted that the hen would perceive the nearest segment as smaller on the ground of objective differences of size and the farthest figure on account of the optical illusion, that it would pick up the grain from the lowest and highest segments without touching the middle one. We did not really expect this experiment to meet with success as the bird could not be supposed to restrain its instinctive impulse excited by the corn so completely that it would not endeavour to seize the grains on the central figure while passing from the lowest to the highest.

Nevertheless the experiment met with an astonishing degree of success. As a rule the hen picked up the grain from the lowest figure

TABLE III. *Experiment on perception of spatial illusion with congruent segments.*

Date	Series	Stimulus	No. of correct reactions	No. of errors	Remarks
Dec. 17	42		10	2	Position 1 and 8
	43		2	0	
	44		2	0	„ 8
	45		2	0	
	46		1	1	„ 10
	47		2	0	„ 11
	48		1	0	
	49		2	0	
Feb. 7	50	□ □	15	2	
	51		2	1	„ 2
	52		5	2	„ 9
	53		2	1	
	54		1	2	
	55		3	2	„ 1
	56		2	1	
Feb. 14	37	□ □	15	5	Analogous position 9
	38	□ □	6	1	
	39		25	9	Position 1 and 8
	40		4	1	
	41		6	2	
Feb. 15	57		2	0	„ 1
	58		12	1	„ 6
	59		4	1	
	60		2	1	
Feb. 28	61		25	9	
	62		8	0	
	63		4	0	
	64		8	1	

first, then passed on to the highest segment, leaving in the majority of instances the food on the middle one untouched[1].

The experiment with hen *A* in Table II is continued in Table III. The control with two congruent and one smaller segment mentioned above is designated.

From the tables it is evident that the bird is not always in a condition favourable to a difficult test which demands so much attention. With animals the attentiveness is probably even more subject to changes and oscillations than with men. Sometimes the bird is satiated and consequently does not exert itself, at another time it is, on the contrary, too greedy and becomes so violently attracted by the stimuli that the training process is thwarted. In many instances it is impossible to determine the cause of the hen's inattentiveness. Whenever it was observed in the course of a training test that the animal was inattentive, the experiment was usually discontinued. The experiments on illusion were only proceeded with after the bird had given fairly satisfactory results in the training tests or repeats. Once the experiment on illusion was begun it was counted among the decisive experiments whether the result was positive or not with reference to the optical illusion.

It may, perhaps, strike one that the series of tests in the experiments on the optical illusion contain so few single tests. This is explained by the fact that the experiments on illusion could not be allowed to become *part of the training*. It was an essential condition that the bird should not become *adjusted* to the *upper* segment by frequent repetitions of illusion tests. Our problem was actually solved by the first reaction and the one immediately following; only to eliminate a possible element of chance was the animal subjected to some further selection tests. Immediately after the conclusion of an experiment on illusion the bird was subjected to some other experiments in order to efface all possible spatial associations with the upper segment. It goes without saying that we took particular care not to influence or disturb the hen during the experiment on illusion. It was allowed to pick wherever it chose, for if by some means or other we had inhibited its erroneous reactions we would have resorted to training, which was just what we sought to avoid[1].

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[1] Hens are able to perceive even small objective differences in size as Katz and myself have first determined (*Zeitschr. f. Psychol.* 1908, L. 93 ff.) and as has been confirmed by Bingham (*Journal of Animal Behavior*, 1913, III. 65 ff.) and by Breed (*idem.* II. 280 ff.). Especially the latter investigator has detected in hens an extraordinary ability to discriminate between differences of size. Cp. Watson, "An Introduction to Comparative Psychology," *Behavior*, New York, 1914, 365 ff. and footnote 16. Further Washburn, *The Animal Mind*, New York, 1917, 222 ff. On the strength of these facts it is safe to assume that in the case of the optical illusion in question the *subjectively* perceived *differences in size* could be easily noticed by the animals.

Not every individual hen is suitable for these experiments. Some are timid, others feed too inconsistently, others again are too inattentive or are easily confused, and, therefore, can only be trained with great difficulty or not at all. Occasionally one meets with birds that refuse to feed in the presence of human beings; these are quite unfit for the free method of training previously described. When after three or four days of training with the circles we did not succeed the animal was discarded. Among seven hens we only found two young birds of 6–8 months who were found easily susceptible to the initial training. We consequently experimented with these birds only on optical illusion in form perception which in both cases led to positive results.

It is possible that someone may urge against these results that the animal may choose the more distant—subjectively smaller—segment, not on account of the optical illusion, but because this segment being farther away than the other *corresponds to a proportionately smaller image on the retina.* At first sight it is not possible to disprove such a criticism, for it is conceivable that with an animal the apparent size of an object only depends on the size of the retinal image, which is obviously proportional to the distance. As the animal is standing 5–8 cms. farther away from the upper segment than from the lower it is possible that the upper segment is conceived as apparently smaller on account of this difference in distance. It is, however, possible to show that in cases in which the retinal image can be made approximately equal the illusion is still present. This was the case in all instances in which the bird stood *immediately in front of the segments.* From this position the two spatial figures should have *appeared* equal, but nevertheless the animal pecked at the correct *i.e.* the *upper* figure. We could further show that even an *actual increase of the upper segment* of 15 degrees, which made the apparent difference between the two congruent segments considerably smaller without annulling it altogether, did not prevent the animal from reacting correctly.

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[1] All experimenters know that animals often behave differently on different days. In a recent publication on visual perception by Bingham, who *inter alia* repeated and confirmed our experiments conducted in collaboration with Katz, we find *e.g.* the following: on six successive days a hen, while being trained to discriminate between square and triangle, had the following correct reactions: 81, 66, 82, 85, 70, 54 per cent ("Visual Perception of the Chick," *Behaviour Monographs*, 1922, IV. No. 4).

4.

In the following experiment we wanted to show that the optical illusions in animals are not necessarily restricted to the illusions so far investigated by us.

The problem which we set ourselves to solve may be stated as follows: Are hens also subject to the optical illusion which involves the underestimation of horizontal lines in comparison with vertical ones?

When of two identical parallelograms one is placed on one of its longer sides and the other next to it on one of its shorter sides the vertical figure appears distinctly larger (Fig. 5).

Again we trained a hen to choose the smaller of two unequal geometrical figures. As soon as the animal was properly trained we placed two parallelograms of *different sizes* in front of the bird and *repeatedly changed their position*. We proceeded similarly as in the case of the unequal segments (see Fig. 1). After completing this training we proceeded to the *experiment on illusion*. In order to obtain the greatest accuracy possible, we arranged the introductory training so that during the final training test, *i.e. the one immediately preceding the experiment on illusion*, the *smaller* parallelogram stood vertically, the larger one *horizontally*. In this way we intended to adjust the hen to the vertical direction, whereas in the experiment on optical illusion it had to pay due regard to the horizontal direction. Even if the animal under these unfavourable conditions had chosen the horizontal figure we would have been justified *a fortiori* in assuming that the hen regularly over-estimates the vertical distance in a similar way as we do.

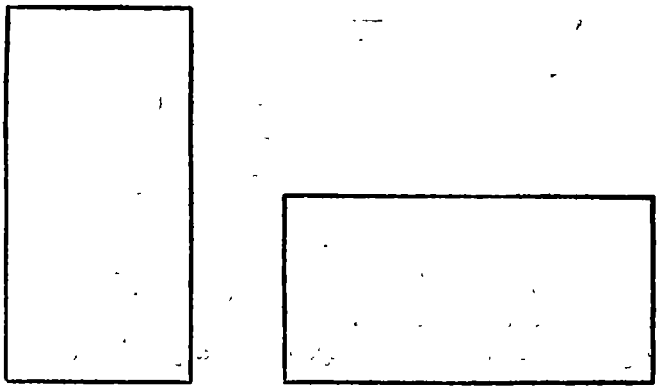


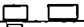
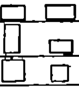
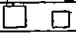
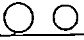
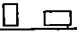
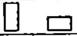
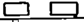
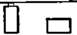
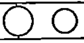
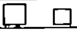
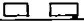
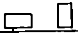
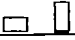
Fig. 5.

The effectiveness of the illusion, which causes vertical lines to appear longer than equal horizontal ones, became quite manifest in this experiment[1].

It was astounding to observe how surely and consistently the animal behaved in the selection. Even if one could reduce the optical illusions to quite elementary factors we could not but marvel at this demonstration of animal behaviour. To the uninitiated it always makes the impression that the animals by some means or other have been trained to react on the chosen figures. We must admit that we ourselves often felt compelled to convince ourselves of the accuracy of our observations and assertions by repeating the experiments again and again.

The following table contains the tests in the experiment on the optical illusion in the comparison of horizontal and vertical distances.

TABLE IV. *Experiment on optical illusion in horizontal and vertical distances or direction.*

Date	Series	Stimulus	No. of correct reactions	No. of errors
Dec. 18	64		4	1
	65		3	2
Feb. 15	66		25	7
	67		19	0
	68		5	0
	69		4	0
	70		8	1
	71		5	0
Feb. 28	72		8	0
	73		6	0
	74		11	4
	75		4	0
	76		4	1

5.

In connection with this investigation we have performed some *comparative experiments upon children.*

Although we were, unfortunately, not in a position to experiment

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[1] In this experiment the figures were placed next to each other with the animal in the middle. In these circumstances it is impossible to accept the hypothesis which explains the illusion by differences in size of the retinal image.

with infants of an age in which speech was as yet undeveloped, we have at least tested only young children, who, on the one hand, could not be expected to know the meaning of the figures and who, on the other hand, could be assumed not to be able to utilise their power of speech to any extent in their activities and observations. The ages of my little observers ranged from 4–6 years. First it was determined whether they could discriminate between geometrical figures of different sizes. As this was the case with everyone of them, the two *congruent* segments were placed before them. All children without exception chose the *subjectively smaller* figure. Similarly without exception the choice fell on the subjectively smaller of two congruent trapezia of which one was white and the other black. The black trapezium being chosen in each case because it appeared smaller. In the course of further experiments, however, it became evident that optical illusions of size, position and direction are not observed by children of four years or slightly older. This may be either because these illusions do not appear so distinctly as in the case of the segments, or because the attention of the children is diverted by other stimuli (oblique lines, etc.). It is also possible that certain illusions require an adjustment, which cannot as yet be attained by such young children. Children of 5½ years on the contrary have observed and judged correctly the optical illusions in the case of equal horizontal and vertical lines and ellipses, and further, the Müller-Lyers illusion. Experiments on illusion with horizontal figures and angles (*e.g.* Zöllner's figure) gave negative results. Further systematic investigation is in course of progress.

We have succeeded in showing that animals are also subject to certain illusions of the senses, and on the evidence it may be concluded that the nature and extent of these illusions cannot differ essentially from those experienced by men. But these illusions are not to be considered unique phenomena, for upon closer scrutiny our observations all prove to be interspersed with more or less obvious illusions. Illusions of distances, angles and perspective are all elements of our perceptions. After establishing the existence of such sense illusions in animals we are justified in infering with some measure of probability that these illusions have played a similar part in the growth in the animal world of perceptions as in ours. When in addition we take into consideration the marked similarity in the phenomena appertaining to the sense of vision[1] we have reasons to assume that the visual sphere of animals resembles in essential characteristics that of men.

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[1] See Hess, Thorndike, Hunter, Lasley, Yerkes, Katz, Révész, Kohler, Szymanski, Turner, Frisch, etc.

The optical illusions with geometrical figures remain to be investigated in animals of higher organization. When we consider, however, that according to our experimentally established data there is no essential difference between the visual qualities of men, mammals and birds, or between their organs of vision, we may conclude with probability that with reference to optical illusions which are psycho-physiologically conditioned no essential differences can exist.

It is perhaps superfluous to state that evidence of the existence of optical illusions in the animal world provides a decisive argument against the judgment theory of optical illusions with geometrical figures, and further that it may render valuable services in the analysis of the psychological act of comparison.

CRITICAL NOTICE

BY GODFREY THOMSON.

Naive Physik. Theoretische und Experimentelle Untersuchungen über die Fähigkeit zu intelligentem Handeln, von OTTO LIPMANN und HELLMUTH BOGEN. Leipzig, Joh. Amb. Barth, 1923. Pp. 155. Price 5 francs (Swiss).

This research on children, which seems to have been at least in some measure instigated by Köhler's experiments on chimpanzees, was carried out from the Institut für angewandte Psychologie in Berlin under the direction of Dr Lipmann, who himself contributes 26 pages of an introductory nature, and an exceedingly useful appendix on other researches into the powers of performance (as distinguished from powers of abstract thought) shown by animals, by normal and defective children, and by adults. The body of the book is by Bogen who carried out the experiments.

By 'naive Physik' the writers mean, roughly, the power of dealing intuitionally with things of the outer world, without the intervention of abstract thought. This sentence may not accurately express what they mean, for it is not a translation of any words of theirs. But it conveys the reviewer's impression after reading their work. The experiments which form the main part of the study were devised to test just this 'naive Physik.' In these experiments a piece of chocolate was enclosed in a ball, and the ball placed in a cage, out of which the child being tested had to get it, with the aid of certain sticks of various shapes.

This cage was of 40 cm. frontage, 35 cm. depth, and 30 cm. height. Its floor sloped towards the child, and it was divided into right and left rooms: but as the division between the rooms did not reach the rear wall, the ball could be sent from the right-hand room (where it was placed) into the left-hand room by poking one of the sticks through the front grating. The cage stood on a table and the furniture was so arranged that the child could not walk around it.

From the left-hand room the ball had to be pushed out of a gap in the rear wall across which gap was a low obstacle like a threshold which the ball had necessarily to surmount. The ball was now outside the cage in a roofless alleyway which ran first to the left and then (rounding the rear left corner of the cage) ran toward the child. The floor of the rear alleyway sloped against the progress of the ball, which could only be moved by poking a stick right through the cage. Thereafter it could be raked toward the child.

The sticks provided were not equally suited for the task, and an important part of the observation was to note which was chosen. The most suitable for most movements had a semicircular crutch (concave upwards) fitting the ball. Another had a *straight* T-piece, others were sharp, or too short, etc.

The ability to extricate the ball quickly and certainly, with the choice of the best weapon and the most suitable application of it, is a case of what the authors call intelligent performance (intelligentes Handeln) as distinguished from intelligent apprehension (intelligentes Erkennen). That these two forms are in fact distinguishable, and by no means highly correlated, is Dr Lipmann's thesis in his introductory chapter. He compares them to the ancient divisions of the arts and the sciences and calls the two types of talent 'technical' and 'gnostic' endowment.

The actual test, in the experimental results, of the independence of the two types will not make a very satisfying impression on those accustomed to large mass-surveys and to exact quantitative measurements, but against obvious and admitted defects must be set the care with which each individual case is studied and the psychological insight of the observers.

Twenty-four cases appear to have been available for the comparison. 'Technical intelligence' is performance in the above cage experiment when points are given for separate piecemeal activities. 'Gnostic intelligence' was arrived at from a combination of teachers' opinions and three tests of inductive thought, immediate memory, and practical judgments. The correlation between the two proved to be only 0·28 by Lipmann's formula, or $\rho = 0{\cdot}5677$ by Spearman's rank formula. Bogen has a few sentences here about the discrepancies which invariably are found between these two formulae. In addition to these 24 cases (all Volksschülerinnen of 12 to 14 years) some contributory evidence is drawn from the cases studied in defective schools. On the whole, the impression left by the reading of the numerous protocols of cases quoted agrees with the result of these correlation calculations, namely, that there was quite a number of children whose practical powers exceeded their ability in school lessons, and that to that extent two types, distinguishable but by no means distinct, can be found.

Comparisons are given between the children and Köhler's chimpanzees. The latter were in certain experiments given the task of reaching an otherwise inaccessible banana by using a stick, or by piling up boxes to stand on, and the like. Bogen is of opinion that the principal difference in intelligent performance between apes and human beings lies in this, that the human being is influenced much more by physical structure, whereas the apes were, according to Köhler, influenced more by the merely optical impression. For example, turning a box on to its corner would, optically, bring a part of the box higher and nearer the banana hanging overhead; but physically the arrangement is unstable. In the case of the children, Dr Bogen stresses the number of movements made for physical rather than optical reasons, as when a child, after putting the crutch-shaped stick through the grating, turns the armpit of the crutch horizontal, though it fits the ball quite as well vertically. The vertical grip is optically as good, but not physically; and it was retained only by less successful subjects. In being willing to push the desired object away from them or sideways (for the purpose of obtaining it ultimately by a round-about route) the children far exceeded the apes, whose impulse to rake the banana in, regardless of insurmountable obstructions, was strong. But Bogen concludes on page 100 (somewhat unexpectedly to me in view of his page 89) that as far as his researches go, no qualitative difference can be shown between the *behaviour* of children and anthropoids in learning (his italics).

Theoretically very interesting, especially to one who like the present reviewer has recently arrived in New York and come within the circle of Thorndike's influence, are the numerous references made to the latter's theories, with which Lipmann and Bogen strongly disagree. Bobertag's German translation (1922) of Thorndike's *Psychology of Education* seems to have stimulated a discussion of his theories which had already begun, for example, in Koffka's *Grundlagen der psychischen Entwickelung* (Osterwieck, 1921).

Thorndike's theory of learning is, in the main, that 'satisfying' results tend to burn in and render predominant the association which led to them, while 'annoying' results do the opposite. With this Law of Effect and with the Law of Exercise, he endeavours to explain how the man of science solves problems correctly, as well as how he behaves in eating, sleeping, resting and playing. 'Satisfying' results are those which imply conduction by neurones which are already in readiness to act, 'annoying' results are those which either imply conduction by unready neurones or refuse conduction to ready units. An educable animal (and pre-eminently man) varies in response to what is (apparently) the same situation (Law of Multiple Response); or even to identically the same outer situation if the inner attitude is different. It attends to bits of the situation (Law of Piecemeal Activity); responds to novel situations by analogy with the most similar past experience; and can gradually shift any response from its original stimulus to any new stimulus by the operation of association and satisfaction (Law of Shifting Associations).

Of this theory (of which of course the above is only a very compressed and crude account) Bogen says, that although it is at first sight alluring in its simplicity, yet on nearer view, so many other mental functions are seen to be included under association, such as modifiability, variability, etc., that the imposing simplicity vanishes. He especially criticises Thorndike's attempt to explain multiple response and multiple causation of response. He quotes Thorndike's sentence: "Original nature seems to decide that the individual will respond *somehow* to certain situations more often than it decides just what he will do." The emphasis, says Bogen, seems to him to lie on the words 'decide' and 'nature.' The 'decision' of 'original nature' is grasping the situation, choice between the means of controlling it, recognition, acceptance and rejection, *i.e.* judgment; in short, the co-operation of intellectual functions.

From his notes, Dr Bogen culls a number of instances at variance with Thorndike's principle of satisfaction and annoyance; a case in which, for example, one 'jab' at the ball fails to send it into compartment II, and then a careful guiding with the crutch is successful. Yet at the second trial the subject proceeds to make "jab" after "jab" in spite of the pleasure previously associated with "guiding" as distinguished from 'jabbing' (subject 18). However, it should perhaps be pointed out that in the first trial, subject 18 failed at a later stage of the run, and so *got no chocolate*; and if there is any trace among children of learning the problem as a whole (such as Peterson found by white rats, and Köhler with his apes) it seems very possible that the whole of the first trial was tinged with disappointment. If I open a game of chess with a reply to a certain gambit, I may have temporary success, and yet reject that reply next time because of ultimate failure. And I may thus reject it even though in fact it is not the cause of that ultimate failure, just as men have rejected setting out for journeys on a Friday.

Anyhow, 'jabbing' is a more primitive response than 'carefully guiding,' and the child (who was becoming excited—the experiment was broken off presently for this reason) was becoming more and more likely, I should say, to 'jab' wildly. The chief difficulty in the way of Thorndike's theory is, in my opinion, the explanation of cases of 'sudden' learning. Such abrupt learning seems to me to depend very considerably on imagery. If I am trying to solve a difficult puzzle of the jig-saw type, I make more certain progress if I record my moves in writing. The animal which does not learn abruptly seems to me to be in a position analogous to the man playing solitaire, say, who does not record his moves. The monkey does sometimes learn abruptly (both Köhler and Yerkes record instances) and the monkey undoubtedly, I should say, has better imagery than has a cat. Yerkes gives strong evidence of the presence of lasting imagery in Dr Hamilton's Skirrl (Behav. Monog. 1916, p. 38).

Surely, however, in spite of the undoubted differences between the Thorndike position and the Lipmann-Bogen position, Thorndike also makes "Schlüssen auf ein inneres Verhalten" (Bogen 99). See for example his *Briefer Course*, pp. 65, 172, *et passim*. The difference between the opponents seems to me to be mainly this, that Thorndike makes a stronger effort to explain things by more economical hypotheses, to push the simple principles of Recency, Frequency, Satisfaction and Annoyance, as far as ever they will go. That is a duty of every scientific worker, to test the hypotheses he makes, as Icarus tested his wings by flying right up to the sun[1]. Icarus failed, and a number of aviators since his time have given their lives. But we now know that a heavier than air machine *can* fly, that no gas bag is necessary. And it may yet be, perhaps, that principles which explain ordinary trial and error may prove successful in the thin air of abstract thought, without any 'powers' called choice, insight, judgment, intellect.

And having thus shown my leanings towards Thorndike's valiant effort, I confess also my inner doubts about the efficiency of some parts of the machine. The problem of abrupt insight after many unsuccessful trials is one obstacle. Another difficulty is in seeing just how Thorndike's Law of Piecemeal Activity, of analysis that is, fits in with Köhler's insistence on the influence of the 'Zueinander,' of the synoptic aspect. The present exceedingly careful and well-described experiments of Dr Bogen, and the theoretical discussions given both by him and Dr Lipmann, are a valuable addition to the controversy, and will command the attention of everyone interested in the problem of learning, and in the nature of ability, especially of that practical concrete ability which our authors call "intelligentes Handeln," and Thorndike calls "thing-thinking."

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[1] I borrow the figure from Eddington's Cardiff address at the British Association.

PUBLICATIONS RECENTLY RECEIVED

Person und Sache: I. Ableitung und Grundlehre. By W. STERN. Leipzig: Barth. 3rd ed. 1923, pp. xx + 434.

Person und Sache: II. Die Menschliche Persönlichkeit. By W. STERN. Leipzig: Barth. 3rd ed. 1923, pp. xx + 267.

Professor Stern (Director of the Psychological Institute of Hamburg) is known outside Germany chiefly for his work in the psychology of individual differences (*Differentielle Psychologie*, 3rd ed. 1921) and for his child study (*Die Psychologie der frühen Kindheit*, 3rd ed. 1923; *Intelligenz der Kinder und Jugendlichen*, 3rd ed. 1920, etc.). As a philosopher he is less well known. The two volumes which are here briefly reviewed expound his philosophical *Weltanschauung* in a system which is throughout teleological—a system which he has aptly termed 'Personalism.' This philosophy is significant for contemporary psychology in that it represents one of the sources of the *Gestalt* theory which at the present moment is dominating German psychology and is beginning to make itself felt in England and in America. The scope of these volumes is not exclusively philosophical. Stern illustrates his position by constant reference to psychological material, and shows step by step how 'Personalism' contributes to the data, the methods, and the interpretations of empirical psychology. (A condensed statement of the relation of 'Personalism' to psychology is published separately: *Die Psychologie und der Personalismus.* Leipzig: Barth, 1917, pp. 54.)

Stern wishes to set aside psycho-physical dualism in favour of a dualistic principle of 'Person' *v.* 'Thing' (*Person-Sache*). We find in the universe only these two types of structures. A 'Person' may be defined as "any existing object which in spite of a multiplicity of parts forms a peculiar Unity of intrinsic and special worth, and as such, in spite of a multiplicity of the functions of its constituent parts, achieves a unitary and purposive self-activity." A 'Thing' conversely is "any existing object which, composed of many parts, forms no genuine unity of a peculiar character or worth, and which, functioning as it does in several parts, achieves no unitary purposive self-activity" (vol. I, p. 16). On the strength of these definitions Stern proceeds with his dichotomy of physical, psychical, and social structures. There are first those which in kind are not simple mechanical creations, composed of separable parts, but which are by nature complex units, indivisible and purposive. Such 'Persons' are individual human beings, families, and states. The 'Thing' has no such self-purpose. It is of a fragmentary nature, and is illustrated by such structures as a pile of sand, a machine, a single chain of ideas, a political club. The contrast is between purposive, sensible totalities and purposeless, mechanical sum-totalities. The antinomy of 'Person' and 'Thing' is resolved to some extent in the conception of hierarchy: each 'Person' is serviceable to a higher, super-ordinate 'Person' just as a 'Thing' is serviceable to the 'Person.'

Volume II is concerned with one special kind of personal structure—that of human personality. The essential concept in this connection (and one of importance for psychology) is that of the *Unitas-Multiplex*. The individual, it is true, is constituted of cells and atoms, ideas and feelings, etc., but is not a simple mosaic of these attributes. He is rather the unitary and indivisible structure which enfolds the multiplicity of such elements within a single purposive whole. Analytical psychology, and the psychology of tests and of correlation, deal only with the *Multiplex*; they have been guilty of neglecting the unity and totality of mental life. It is precisely this challenge which contemporary psychology in Germany is endeavouring to meet.

The third volume of this series (*Person und Sache: III. Wertlehre*) is now in press. It deals with the ethical reflections of 'Personalism.' Included in this volume will be found a significant chapter on the modes of self-expression which involve the action of the *total* personality. Stern is a pioneer in the 'psychology of total structures' in much the same way as he was in the analytical method twenty years ago. He is, perhaps, the first psychologist to define clearly the nature of these two fields, and to divide his work equally between them.

GORDON W. ALLPORT.

Psychology and Primitive Culture. By F. C. BARTLETT, M.A. Cambridge: at the University Press, 1923. Pp. ix + 294. Price 8*s.* 6*d.* net.

This book is a contribution of great interest and significance both to Social Psychology and to Folk Psychology. The author has suggested lines of enquiry to a large extent fresh, and has marked out an unquestionably fertile field for future psychological investigation. If he had done nothing more, the book would have justified its existence. But he has done much more. He has indicated the best and most hopeful lines of approach to the main problems, and he has brought us very appreciably nearer a solution of these problems.

The first two chapters are preliminary, but very valuable in defining the point of view. At the outset we have placed before us in the clearest fashion the nature of the psychological problems involved in primitive social life and culture, the inter-relations of these problems with one another, and the relation of the field of study so marked off to other fields of study, and particularly to Sociology. This is followed by an attempt to place the whole enquiry with relation to General Psychology. A start is made from the instinctive responses of man, as enumerated, for example, by McDougall. The first valuable point made is the interdependence and inter-relation of the underlying tendencies. This is a point too apt to be forgotten, and the result is often a very arbitrary and onesided account of the concrete behaviour of the human being. The author in specifying the instinctive tendencies which are social, is inclined to include play. We doubt whether this is legitimate. Of course, there are social types and forms of play, and play exercises important influences in social life, but whether in itself it is social is a much more doubtful question.

The basic scheme of the book, which might also be said to represent the ground-plan of a Social Psychology, is then indicated. This shows four main lines of enquiry: (1) the fundamental forms of social relationship as expressed in "the tendency towards primitive comradeship," "the tendency of assertiveness," and "the tendency of submissiveness"; (2) other fundamental responses having a social reference, of which the chief are the tendencies towards conservation and towards constructiveness; (3) individual instinctive responses, the social effects of which depend on the fundamental forms of social relationship; and (4) "group difference tendencies," which show themselves in connection with the institutions and conventions of a community. The various factors in social life, as thus classified, are brought out and illustrated by the subsequent study in the rest of the book of folk stories and legends, the contact of peoples, the transmission of culture by borrowing, the diffusion of culture, and the elaboration of culture, the point of view and line of treatment being finally justified in the concluding chapter.

This is the briefest kind of summary of a book which presents many interesting features. Not the least of these is the threefold classification on a psychological basis of the fundamental forms of social relationship. This must be regarded as a very valuable contribution to Social Psychology, and can be fruitfully developed in many directions. The notion of "group difference tendencies" is another important and suggestive contribution. The discussion of the conflict and mutual reinforcement of instinctive tendencies is yet another valuable contribution. The analogy of the associated group phenomena to phenomena of individual psychology, studied and emphasized by the psychoanalytic school, is worked out in so illuminating a way as to carry immediate conviction of the value of the method of approach adopted.

The sanity of the attitude throughout is the chief impression the book leaves behind. There is nothing of that wild speculation, nothing of that special pleading, nothing of that manipulation and frequent distortion of the facts so as to support preconceived theories, which have so often characterized works treating of the same and kindred topics. The present book is, we hope, but an earnest of a fuller and more detailed treatment by the same author, from the same point of view, and by the same method, of fundamental problems of Social Psychology.

J. D.

Intelligenzprüfung und Psychologische Berufsberatung. Von Dr RUDOLF LÄMMEL. München und Berlin: R. Oldenburg, 1923. S. vi + 193.

This monograph aims at demonstrating the desirability of vocational guidance. It is concerned rather with a general plea for the institution of psychological vocational guidance on a large scale, than with a detailed account and a statistical treatment of intelligence testing. A scheme is given for the examination of certain arbitrarily selected mental 'functions,' such as memory, mechanical and artistic ability, etc.; and a useful means of presenting a rapid survey of the individual range of these functions supplied in the 'Ingenogramm.' For those who know little or nothing of vocational testing, this monograph will be useful; but its reasoning is rather vague and its statements sometimes ill-founded.

The Will-Temperament and its Testing. By Professor JUNE E. DOWNEY. World Book Company, Yonkers, New York, 1923. Pp. 339. Price $1.85.

After a reference to the study of personality by James, Jung, Rosanoff, Davenport and others, there follows an account of personality traits and their attempted measurement. Professor Downey's own contribution to this difficult task is her Will-Temperament Tests which are labelled, Speed of Movement, Freedom from Load, Flexibility, Speed of Decision; Motor Impulsion, Reaction to Contradiction, Resistance to Opposition, Finality of Judgment; Motor Inhibition, Interest in Detail, Coordination of Impulses, Volitional Perseveration. Most of these tests involve the study of handwriting under various conditions, as the author believes temperament to be largely determined by the tendency of nerve energy to express itself immediately in motor reaction. Care must be taken, however, not to conclude too much from this somewhat one-sided approach. Thus Volitional Perseveration is assumed to be the trait revealed by a prolonged effort to disguise handwriting. One would, however, desire to see this somewhat elusive trait studied from various aspects. Especially interesting is the testing of the tests by the method of self-rating by highly intelligent subjects and by the testing of psychopathic subjects. It is a book which can be strongly recommended to the discriminating reader.

The Unconscious. By I. LEVINE, M.A. London: Leonard Parsons, 1923. Pp. 215. Price 7*s.* 6*d.* net.

This volume is announced as "An Introduction to Freudian Psychology." The trouble with brief introductions to extensive and complicated subjects is that they are usually more helpful to the advanced student than to the beginner. To anyone acquainted with Freud's own writings Mr Levine's book will prove a most useful summary of the main points and theories; but it is questionable whether one who seeks an 'introduction' would not find it better to go direct to Freud's "Introductory Lectures on Psycho-Analysis" than to this very compressed attempt "to study the pure theory on which psycho-analysis rests."

In spite of a disjointed style, the book will be welcomed as a careful attempt to summarize the Freudian psychology; and in this respect it contrasts very favourably with the sort of book which aims at defending the almost papal infallibility of Freud, on the one hand, and the sort of book which aims at the destruction of Freud and all his works as a kind of psychological "abomination of desolation," on the other hand.

Some Applications of Psycho-analysis. By Dr OSKAR PFISTER. London: George Allen and Unwin, Ltd., 1923. Pp. 352. Price 16*s.* net.

Dr Pfister made a valuable contribution to Psycho-analytic literature in his volume *The Psycho-analytic Method.* The present book does not add appreciably to knowledge of the subject, or to the author's reputation. Indeed, the impression conveyed by a perusal of these pages is that Dr Pfister has come to regard himself as an authority on psycho-analysis and that it is necessary therefore for him to keep himself before the public, even though he has no material of particular importance to offer.

Three Chapters on the Nature of Mind. By BERNARD BOSANQUET, F.B.A. London: Macmillan & Co. 1923. Pp. vii + 159. 6*s.* net.

These three brief essays, published after the death of Dr Bosanquet, though they deal with mind mainly from the point of view of the abstract philosopher, yet contain much that the psychologist will find both interesting and significant. Three views of mind are presented. The first is said to be the biographer's, and treats mind as a consciousness containing many thoughts, feelings and the like, and only to be described as a whole. The second is the Brentano-Meinong view, where consciousness is distinguished by its relation to an object, and the factors involved in this relation are given detailed analytic consideration. The third is "the Russell mind," which is simply a complex mass of habits. The book represents part of a larger treatise the rest of which, unhappily, was never completed.

Public Opinion in War and Peace. By A. L. LOWELL, President of Harvard University, Cambridge, U.S.A. Harvard University Press, 1923. Pp. xi + 303. Price 10*s.* 6*d.* net.

This is an interesting, but not a *very* interesting study in social psychology. President Lowell considers the formation of personal and 'collective opinion,' how it is influenced by various social factors, and its expression in and through political parties. He deals more briefly with the outstanding features of public opinion during and shortly after the late war, in the countries mainly concerned. The whole treatment is thoughtful, balanced, and clear. But it is not strikingly original.

The Boyhood Consciousness of Christ. By Rev. P. J. TEMPLE. New York: The Macmillan Company, 1922. Pp. xi + 244.

Proceedings of the Aristotelian Society, N.S., vol. XXIII. London: Williams and Norgate, 1923. Pp. 289. Price 25*s.* net.

Relativity, Logic and Mysticism. Papers for discussion at the Joint Session of the Aristotelian Society, the Mind Association, and the Scots Philosophical Club, Durham, July 13–16, 1923. London: Williams and Norgate, 1923. Pp. 184. Price 15*s.* net.

A Commentary to Kant's Critique of Pure Reason. By Professor NORMAN KEMP SMITH. 2nd ed. revised and enlarged. London: Macmillan & Co., 1923. Pp. lxi + 651. Price 25*s.* net.

NOTES ON RECENT PERIODICALS

Psychologische Forschung.

Bd. II, Hefte 3 and 4, 1923.

Die Höchste Gottheit bei den kulturarmen Volkern. (K. Th. Preuss.)

The author makes use of material gained from certain primitive tribes of South and Central America in an attempt to elucidate the peculiar position of the "chief god" with respect to that of other gods and demons.

He considers that the presence of a "chief god" and his characteristics in these undeveloped types of religion prove that the idea of a supreme power ruling and personifying the world was coeval with the multiple personifications of the various natural phenomena. The power of influencing these latter and of maintaining order and regularity was regarded as having been placed in men's hands by a retired "chief god," and lay in their ritual and magical acts. A further conclusion emphasizes the close relation of myth to religion.

Studien zur Untersuchung der Intelligenz bei einem Fall von Seelenblindheit. (W. Benary.)

The eighth of a series of psychological analyses of cases of brain pathology based on examination of those suffering from injuries to the brain, and carried out in the Frankfort institute for such research.

This is a case of injury to the outer central visual tract in the left occipital lobe and a lesion of the left cerebellum, resulting in a severe "apperceptive mental blindness," in the direction of a loss of a sense of form, of power of localization, of perception of movement and tactile space. For these the patient compensated to some extent by movements of hand, feet or head, inhibition of which rendered impossible many acts of judgment.

A close study of his mental powers and processes was carried out in a series of tests extending over some years, in order to throw light on the question as to what constitutes "intelligence." The patient showed no signs of stupidity or lack of attention or concentration. His "general intelligence" was of high grade in some directions, defective in others. The enquiry into these differences led to the conclusion (1) that all defects were due to a disturbance of the visual tract, even in such performances, as judgment of time; (2) that this was not due to the loss of visual images in the ordinary sense, but to a loss of spatial image or spatial structure to which the absence of visual images must be attributed; (3) this spatial imagery is formed simultaneously with visual imagery and gives structure to the latter, hence without it visual imagery is chaotic and of no functional use.

Untersuchungen über eine einfache natürliche Reaktionstatigkeit. (J. S. Szymanski.)

The "simple natural reaction" of scratching was chosen for research into the instinctive, habitual or intelligent factors involved, and the experiments were carried out on normal adults, infants, and congenital idiots.

As a result the author arrives at the following definition: The simple human act is a habitual series of movements on the basis of an inborn disposition. This series of movements is regulated both by modifications imposed by the range and duration of the reaction (not conditioned merely by the strength of the stimulus) and by adjustment to ever new and changing exterior conditions by means of intelligence, whose activity usually takes a subconscious course."

Über einen Apparat zur Messung von Tonintensitaten. (Kurt Lewin.)

A description of the apparatus used in the experiments described in the three following articles. It allows a simple determination of the relationship of the amplitude of tones of equal pitch and of the intensity of a single tone within a chord.

Über den Einfluss von Interferenzröhren auf die Intensität obertonfreier Töne. (Kurt Lewin.)

In his experiments on the diminishing intensity of a tone freed from over-tones as a result of the addition of further interference-pipes, the experimenter found that the influence of a single interference-pipe is dependent on the number, position and length of those already effective.

Über Höhenänderungen bei Schwebungen. (Margarete Eberhardt.)

(1) Experiments carried out with the above apparatus showed that where A and B are two tones of varying intensity and of unlike amplitude, of different frequencies m and n, the following Helmholtz formulae are only true where $m - n$ is small in comparison with m or n, a limitation laid down by Helmholtz himself:

at the maximum $$\frac{mA + nB}{A + B} = n + \frac{(m - n)A}{A + B}$$

at the minimum $$\frac{mA - nB}{A - B} = n + \frac{(m - n)A}{A - B}.$$

(2) Subjective observations of the effect of alterations in intensity of two primary tones, where the difference between these did not exceed eight vibration frequencies confirmed the above, *i.e.* only one tone of constant pitch is heard lying between the two primary tones. If the latter are of equal strength it lies near a middle point, if they are unequal, it lies nearer the louder. But the extension of the experiments to tones of greater frequency differences showed that *two* tones could be distinguished, one nearly equalling the pitch of the louder primary tone, the second two to five frequencies from this, between the two primary tones. This result is, of course, incompatible with the Helmholtz theory of specific energy, unless this is supplemented in some way, as by Stumpf and Röhler.

Über die phanomale Höhe und Stärke von Teiltönen. (Margarete Eberhardt.)

As a result of a series of experiments on (1) ability to determine subjectively the pitch of one out of several combined tones, (2) subjective determination of the intensity of one out of two, three or more combined tones as compared with its objective value, the author finds that while (1) gives correct results in (2) only a part of the objective intensity of the tone distinguished is perceived by the subject, the amount being constant for one, but very varied for different subjects; that it is greatly influenced by variation of intensity of the remaining tones; that such a part tone can still be detected when its objective intensity is very considerably diminished, and that it still 'colours' the whole sound when too weak to be so detected; further, that there is a subjective variation of the pitch of such an 'analysed' tone.

The conclusion drawn is that it is no longer possible to hold the older theory that the phenomenal unity of sounds heard in combination is only apparent; only a part of such a combined sound *can* be analysed, the diminished intensity of the analysed sound being due to the inclusion of the remainder of its intensity in the unanalysable mass. Proceeding further, the author finds the Helmholtz theory as it stands insufficient to explain the above phenomena.

Psychologische Bemerkungen zu zwei Werken der neueren Kunstgeschicht. (G. J. von Allesch.)

An attempt to contribute to a scientific understanding of the essential nature of art. The paper proceeds from the point of view of the alteration brought about in appreciation and understanding of any particular work of art as a whole by an understanding of one particular detail, and distinguishes three types of relationship between the isolated forms or details and the meaning of the whole.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL SECTION.

February 23, 1924.	Introspection and the Methods of Psychology, by K. Koffka.
	Personality as an Object of Perception, by G. W. Allport.
	Meaning in Psychology, by Morton Prince.

Demonstrations.

	A discrimination box for animal experiments, by V. D. Hadley.
	Photographs of the movements of telephone diaphragms, by H. Banister.
	Movement over the blind spot: (i) W. J. H. Sprott, (ii) G. D. Morgan. A Hudson Davies.
	The legibility of type, by R. L. Pyke.
	Long spells of repetitive work, by M. D. Vernon.
	Thermionic valves in experiments on audition, by H. Banister.
March 22, 1924.	The Psychological Method in Anthropology, by W. E. Armstrong.
	The Motor Aspect of Rhythm, by H. E. O. James.

SECTIONAL MEETINGS.

Aesthetics Section.

March 28, 1924.	The Relevance of Psycho-analysis to Art Criticism, by A. M. Bodkin.

Education Section.

January 7, 1924.	Recent Advances in the Relation of Psycho-analysis to Education, by J. Glover.
February 11, 1924.	Intelligence Tests in City and Urban Schools, by T. P. Tomlinson.
March 10, 1924.	The Nature and Validity of Subjective Estimates of Intelligence, by D. W. Oates.

Industrial Section.

February 28, 1924.	The Psychological aspect of Occupational Diseases, by May Smith.

Medical Section.

January 23, 1924.	Primitive Mentality and the Unconscious, by H. G. Baynes.
February 27, 1924.	Certain Symptoms of the Traumatic Neuroses: The Place of the Physician in their Aetiology and some points in their Treatment, by A. C. Wilson.
March 11, 1924.	Meaning and 'Setting" in relation to Pathological States—a Theory of Progress, etc., by Morton Prince.

COMMITTEE FOR RESEARCH IN EDUCATION

With a view to facilitating the general co-ordination of educational research, the Committee recently made an inquiry as to researches in progress (or planned) at the various University Psychological Laboratories and Education Departments. The following is a summary of the replies received.

Researches in progress or planned, December, 1923.

MENTAL TESTS (GENERAL).

1. A Survey of Mental Tests. (Bedford College.)
2. Practical Use of Intelligence Tests in Secondary Schools. (University of Leeds.)
3. Effects of Coaching in Intelligence Tests. (University of Leeds.)
4. Intelligence of sick children in hospital, in connection with an investigation under the Medical Research Council. (University of Glasgow.)
5. The comparative diagnostic value of tests of intelligence and tests of knowledge. (University of Sheffield.)
6. Tests of General Intelligence:
   - (*a*) Standards for various ages and occupations.
   - (*b*) Group tests of intelligence for higher posts.
   - (*c*) Oral tests of intelligence.
7. Classification of occupations according to intelligence required. Study of distribution of intelligence among the general population both juvenile and adult, with special reference to vocational requirements. (6 and 7, National Institute of Industrial Psychology.)
8. Terman Tests of Intelligence for individual applicants. (National Institute of Industrial Psychology.)
9. Statistical analysis of harder group tests for intelligence. (National Institute of Industrial Psychology.)

MENTAL TESTS (SPECIAL).

1. Practical ability. (Bedford College.)
2. Tests of mathematical ability. (University of Leeds.)
3. Vocational tests in Science. (University of Leeds).
4. Vocational tests in Music. (University of Leeds.)
5. Vocational tests in:
   - (*a*) Shorthand typists.
   - (*b*) Book-keeping and accountancy.
   - (*c*) Dressmaking.
   - (*d*) Engineering.
   - (*e*) Value and improvability of tests of motor, manual and constructive ability.
   - (*f*) Tests for aesthetic appreciation and artistic ability.

6. Job analysis for clerical work.
7. The reliability and technique of interviewing.
8. Tests for constructive imagination. (5, 6, 7 and 8 National Institute of Industrial Psychology.)

OTHER STUDIES WITH EDUCATIONAL BEARINGS.

1. Extent of individual variation in similar tasks.
2. Group tests of geographical knowledge. (1 and 2, University of Sheffield.)
3. Effect of games on school work.
4. The psycho-galvanic reflex. (3 and 4 Leeds.)
5. A study of character qualities in children.
6. A study of difficult children. (5 and 6 King's College.)
7. The measurement of temperamental qualities.
8. The psycho-galvanic reflex, and its possibilities for testing temperament. (7 and 8, National Institute of Industrial Psychology.)
9. The supernormal child. (Glasgow.)
10. An enquiry into the mental images of school children with special reference to the theories of Jaensch. (Cambridge.)

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